remark expresses the nature of the book. The author is satisfied with seeing; she has made no new routes and collected no new information, and some of her statements, such as that the egg of the Kiwi is as large as the adult bird, are untrustworthy; but she will doubtless feel repaid if her book leads others to visit Westland, and share her keen enjoyment of that beautiful land.

THE REV. F. J. JERVIS-SMITH, F.R.S.

BY the death of the Rev. Frederick Jervis-Smith on August 23, at sixty-three years of age, the world of science has lost an original and acute thinker and a man who had a genius for designing and constructing instruments of delicacy and precision. Trained as a mechanical engineer, he gave up the calling of his choice, went to Oxford and entered the Church for family reasons. The only son of the Rev. Prebendary Frederick Smith, of Taunton, he became the patron of the living of St. John's, Taunton, and was vicar for a few years. But he recognised that his real gifts were for science, and he took his workshop to Oxford, where he became Millard lecturer in experimental mechanics at Trinity College.

The teaching laboratory in Trinity fitted up by Jervis-Smith was worked in connection with the chemical laboratory in Balliol, and afterwards with the laboratory in St. John's, fitted up by Bosanquet, the three laboratories being close together. The passage opened between Trinity and Balliol in 1879 was known as "the scientific frontier."

In the Millard Laboratory Jervis-Smith constructed many of his well-known instruments, among which special mention must be made of his electric chronograph. Instead of the ordinary device of a heavy pendulum or rod falling under the accelerating force of gravity, Jervis-Smith made a carriage to run down rails so inclined that the velocity became constant after a certain travel. This carriage carried a smoked surface on which electromagnetic styli made their trace, as well as a vibrating tuning-fork. This uniformity of movement greatly simplified the conversion of the distance between the marks of the styli into time. The styli and the electromagnets were very small, and the retardation of the release on breaking the circuits was made by an ingenious system of winding the coils, both very small and nearly uniform.

Jervis-Smith had intended to use his chronograph in the investigation of the changes of velocity in the propagation of the flame in the explosion of gases; and, indeed, he made several sets of experiments on the propagation of the explosion of electrolytic gas under pressure in steel pipes, but he returned to the improvement of the instrument. Prof. H. B. Dixon carried out all his later researches on the velocity of the explosion-wave in gases with the help of electromagnetic styli constructed by Jervis-Smith. This chronograph has been largely used for measuring the flight of projectiles. Of his other instruments, the best known are the dynamometer and the integrator, but many of his ideas have been adopted in other measuring and recording instruments.

Jervis-Smith was endeared to his friends by his simple character, his dry humour, and the kindness of his heart. He would put himself to endless trouble to help a friend in any experimental problem, and he always managed to convey the idea that one was doing him a service by asking for his help. His skill and courage in saving life on the river at Oxford were recognised by the award to him of the Royal Humane Society's medal. He married Miss Annie Eyton Taylor, and leaves her and one son to mourn his loss.

THE BRITISH ASSOCIATION AT PORTSMOUTH.

THE week's meeting of the British Association at Portsmouth has now drawn to a close, and some general impressions of the gathering may not be out of place. In the first place, the weather conditions in Portsmouth, as in most other parts of the United Kingdom, have been exceptional as regards absence of rain and high thermometric readings. Only on one day has rain fallen during the whole of the week, which, speaking off-hand, has probably been a very rare occurrence even during the eighty odd years of the association's existence.

The attendance has been low, which is to be deplored, as on the whole the standard of the scientific work has been high. The address of the president (reported in full in our last number) was delivered in the Town Hall. The attendance at the sections, which began their work on Thursday morning, was not large, but the presidential addresses were of great interest and value. The largest number of members seems of recent years to be attracted to the sections dealing with the subjects which come into everyday life, and of which the "man in the street" is conscious. Thus in the economic and the education sections the speakers had fair audiences. Agriculture (Sub-section K) also appealed to a good many members.

With the fine weather, naturally the garden-parties were much appreciated, and on Saturday the all-day excursions were practically all up to the limit as regards numbers. One party was conducted over Goodwood House, and entertained at tea by Mr. Hussey Freke, the agent to the Duke of Richmond; another was the guest of the Duke of Norfolk at Arundel Castle. Other excursions were in the Isle of Wight and to the New Forest. It conduced much to the pleasantness of each excursion that certain gentlemen gave their local knowledge and services to act as guides to the several parties.

There were, as usual, two evening lectures—the first, on "The Physiology of Submarine Work," by Dr. Leonard Hill, attracting a fair audience; the second was by Prof. A. C. Seward on "Links with the Past in the Plant World."

During the meeting various lectures of considerable interest were arranged, and it seemed a pity that they were not more widely advertised. Mr. F. Enock lectured on "Fairy-flies," and Dr. Francis Darwin on "The Balance-sheet of a Plant."

There was a great demand for tickets for the naval display on Monday afternoon, which gave the visitors an insight into the mass of detail and training required by naval commanders of the present day. The party was taken on board the battleship *Revenge*, and watched an attack by torpedo-boat destroyers and four or five submarines specially told off for the occasion.

Most sections finished their work on Tuesday, but a few energetic ones had material to keep them going until Wednesday.

The meeting next year is to be held at Dundee, beginning on September 4, and the president will be Prof. E. A. Schäfer, F.R.S. The invitation of the City and University of Birmingham to meet there in 1913 was unanimously accepted at the council meeting on September 1. The council also resolved by a majority to recommend that agriculture be constituted a separate section of the association, and this recommendation has been adopted by the General Committee and the Committee of Recommendations, so that there will be twelve sections in future, agriculture being Section M.

SECTION C.

GEOLOGY.

Opening Address by Alfred Harker, M.A., F.R.S., President of the Section.

Some Aspects of Modern Petrology.

In accordance with the custom which permits the occupant of this chair to open the proceedings with observations on some selected subject, I wish to invite your attention to certain points concerning the genetic relations of igneous rocks. The considerations which I shall have to lay before you will be in some measure tentative and incomplete; and indeed, apart from personal shortcomings, this character must necessarily attach to any discussion of the subject which I have chosen. For petrology is at the present time in a state of transition—the transition, namely, from a merely descriptive to an inductive science—and at such a time wide differences of opinion are inevitable. If I should seem to do less than justice to some views which I do not share, I hope this fault will be attributed to the limitations of time and space, not to any intention of abusing the brief authority with which I find myself invested.

The application of microscopical and special optical methods, initiated some fifty years ago by Dr. Sorby, gave a powerful impetus to the study of the mineral constitution and minute structure of rocks, and has largely determined the course of petrological research since that epoch. For Sorby himself observation was a means to an end. His interest was in the conclusions which he was thus enabled to reach relative to the conditions under which the rocks were formed, and his contributions to this problem will always rank among the classics of geology. The great majority of his followers, however, have been content to record and compare the results of observation without pushing their inquiries farther; and indeed the name "petrography," often applied to this line of research, correctly denotes its purely descriptive nature. A very large body of facts has now been brought together, and may be found, collated and systematised by a master-hand, in the monumental work of Rosenbusch. Beyond their intrinsic interest, the results thus placed on record must be of the highest value as furnishing one of the bases upon which may eventually be erected a coherent science of igneous rocks and igneous activity.

In earnest of this promise, recent years have witnessed a very marked revival of interest in what we must call at present the more speculative aspects of petrology. This manifests itself on the side of the petrographer in a growing disposition to seek a rational interpretation of his observations in the light of known physical principles, and on the side of the field geologist in a more constant regard for the distribution, mutual associations, and mode of occurrence of igneous rocks. I will add, as another hopeful sign of the times, a decided *rapprochement* between the laboratory and the field, too often treated in practice as distinct departments.

As regards the former, the movement which I have noticed is merely a return to the standpoint of Sorby, the father of modern petrology. It is true indeed that, before his time, the problem of the origin of igneous rocks had engaged the ingenuity of Scrope and Darwin, of Bunsen and Durocher, and many others; and the bold speculations of the heroic days of geology have justly exercised a lasting influence. The petrologist of to-day, however, has at his command a much ampler range of information than was possessed by his predecessors. In addition to the rich store of petrographical data already mentioned, he can press into service on the one hand the results of physical chemistry and on the other much additional knowledge which has been gathered concerning the structure of the earth's crust and the distribution of various rock-types, both in space and in time. Either of these branches of the subject would furnish material for a much longer address than my assurance could venture or your complacence would endure. I have chosen the geographical aspect of petrology; but, before proceeding to this, I will say a few words concerning the experimental side.

Data from the Experimental Side.

That the modern developments of physical chemistry, starting from the phase rule of Willard Gibbs, must in theory furnish all that is necessary to elucidate the crystallisation of igneous rock-magmas, has long been perceived by some petrologists. This recognition is in itself an advance. Natural rock-magmas, however, are far more complex solutions than those which chemists have employed in working out their laws, and the problem in its entirety is of a kind almost to daunt inquiry. Despite the courageous attempt made by Prof. Vogt, whose enthusiastic lead has done so much to inspire interest in the subject, it seems clear that the application of the laws of chemistry to the particular class of cases with which the petrologist is concerned demands as a prerequisite a large amount of experimental work in the laboratory. The high melting-points of the rock-forming minerals, their extreme viscosity, and other specific properties render such work extremely difficult and laborious. That most of the practical difficulties have now been overcome is due in the first place to Dr. A. L. Day and his colleagues of the Geophysical Laboratory at Washington, who have thus opened out what is virtually a new field of investigation. The methods of high temperature measurement have been perfected and the thermometric scale standardised up to 1550° C., thus embracing the whole range of rock-formation. Calorimetric measurements have been so far improved that it is now possible, for instance, to determine specific heats, even in the highest part of this range, with an accuracy ten times greater than has hitherto been usual at ordinary temperatures. Incidentally there has been, in the hands of Mr. F. E. Wright, a notable enlargement of the scope of ordinary petrographical methods, since it has been found necessary to devise special means of measuring with precision the crystallographic and optical constants of very minute crystals.

The American chemists have already determined the temperature-range of stability of numerous rock-forming minerals. Beginning with the simpler cases and working always with chemically pure material, they have established quantitatively the mutual relations of the various possible forms in a number of two-component systems and in one of three components. So far as these instances go, the mutual lowering of melting-points in a silicate-magma is now a matter of precise measurement, and it is no longer inferred, but demonstrated, that the order of crystallisation of the minerals depends upon their relative proportions in the magma. The perfect isomorphism of the plagioclase felspars has been finally established, and a certain degree of solid solution between quite different minerals has furnished the explanation of some apparent anomalies, such, for instance, as the variable composition of the mineral pyrrhotite. As a single illustration of how these investigations in the laboratory provide the working petrologist with new instruments of research, I will cite the conception of a geological temperature-scale, the fixed points on which are given by the temperature-limits of stability of various minerals. It is often possible, for example, to ascertain whether quartz in a given rock has crystallised above or below 575° C., this being the inversion-point between the α- and β- forms of the mineral. At about 800° there is another inversion-point, above which quartz is no longer stable, but gives place to cristobalite. In like manner we know that wollastonite in a rock must have crystallised below 1190°, pyrites below 450°, and so for other cases. We may confidently hope that, with the aid of such data, we shall soon be enabled, by simple inspection, to lay down in degrees the temperature-range of crystallisation of a given igneous rock.

There are now several laboratories where high-temperature research, of the rigorous order indicated, is being carried out; but the work is peculiarly arduous, and results come slowly. Some branches of the inquiry, notably those involving high pressures, and again the investigation of systems into which volatile components enter, are as yet virtually untouched. For these reasons it would be premature to hazard at this stage any more detailed forecast of the services to be rendered to petrology by synthetic experiment. I will accordingly leave this attractive subject, and pass on from the laboratory to the field.

Geographical Distribution of Igneous Rocks.

Here the existing situation is very different. Instead of following out definite lines already laid down, we are concerned in reducing to order a great mass of discrete facts drawn from many sources. The facts which enter into consideration are those touching the distribution of various

igneous rocks in time, in space, and in environment, including their relation to tectonic features; the mutual association of different rock-types and any indications of law in the order of their intrusion or extrusion; and, in short, all observable relations which may be presumed to have a genetic significance. The digestion of this mass of data has already led to certain generalisations, some of which are accepted by almost all petrologists, while others must be regarded as still on their trial.

Of the former kind is the conception of petrographical provinces, which was put forward by Prof. Judd twenty-five years ago, and has exercised a profound influence on the trend of petrological speculation. It is now well established that we can recognise more or less clearly defined tracts, within which the igneous rocks, belonging to a given period of igneous activity, present a certain community of petrographical characters, traceable through all their diversity or at least obscured only in some of the more extreme members of the assemblage. Further, that a province possessing an individuality of this kind may differ widely in this respect from a neighbouring province of like date; while, on the other hand, a striking similarity may exist between provinces widely separated in situation or in age. It is natural to attribute community of chemical and mineralogical characters among associated rocks to community of origin. The simplest hypothesis is that which supposes all the igneous rocks of a given province to be derived by processes of differentiation from a single parent-magma. This may be conceived, for the sake of simplicity, as initially homogeneous, though doubtless some of the causes which contribute to promote heterogeneity were operative from the earliest stage. Granted this hypothesis, it follows that the points of resemblance among the rocks of a province will indicate the nature of the common parent-magma, while the points of diversity will throw light on the causes of differentiation. The observed sequence in time of the various associated rock-types will also have an evident significance, especially if, as there are good reasons for believing, differentiation in igneous rock-magmas is largely bound up with progressive crystallisation. Those petrologists, on the other hand, who attach importance to the absorption or "assimilation" of solid rock-matter by molten magmas, are bound to consider both the nature of the chemical variation and the local distribution of the different types with constant reference to the composition of the country-rocks. The balance of opinion, and I think of argument, would assign the variation, at least in the main, to differentiation; and there are well-known principles, chemical and mechanical, which theoretically must operate to produce a diversity of ultimate products from a magma originally uniform. How far these principles are in practice adequate to the demands which have been made on them, is a question not to be finally resolved without quantitative knowledge which is still a desideratum. Experiment may in time come to our aid. My design to-day is rather to offer some remarks upon a distinct, though allied, problem—viz., that presented by the petrographical provinces themselves.

The geographical distribution of different kinds of igneous rocks long ago engaged the attention of Humboldt, Boué, and other geologists, and the subject has always possessed a certain interest in view of the association of most metalliferous deposits with igneous rocks. It has, however, acquired a new importance in recent years in connection with questions of petrogenesis which are still under discussion. The problem is, in brief, to account for the existence of petrographical provinces and for the observed facts relative to their distribution. One theory, advocated especially by Dr. G. F. Becker, invokes primæval differences in composition between different parts of the globe, which have persisted throughout geological time. It involves the hypothesis that igneous rock-magmas result from the refusion of pre-existing rocks within a limited area. Indeed Becker discards altogether the doctrine of differentiation, and conceives the varied assemblage of rocks in a given province as produced by admixture from a certain number of primitive types. These, he says, should be recognisable by their wide distribution and constant character. It is clear, however, that, on the hypothesis of admixture, the primitive types must be those of extreme composition. These are, in fact, always the rarest and the most variable, pointing not to admixture but to differentiation as the cause of the diversity. A theory which attributes the special characteristics of petrographical provinces to permanent heterogeneity in the composition of the globe is difficult to reconcile with the small extent and sharp definition of some strongly characterised provinces, such as that of Assynt or of the Bohemian Mittelgebirge. A more fatal objection is that petrographical provinces are not in fact permanent. A good illustration is afforded by the midland valley of Scotland, an area our knowledge of which has been much enlarged by the recent work of the Geological Survey. It was the theatre of igneous activity in Lower Old Red Sandstone times and again in the Carboniferous, but, in respect of mineralogical and chemical composition, the two suites of rocks present a striking contrast. The Old Red Sandstone lavas are mostly andesites, though ranging from basalts on the one hand to rhyolites on the other, and the associated intrusions are mainly of diorite, quartz-diorite, and granite, with porphyrites and other dyke-rocks. In the Carboniferous, on the other hand, we find porphyritic basalts, mugearites, and trachytes (including phonolitic types), with picrites, teschenites, monchiquites, orthophyres, and other allied rocks. It would be possible to cite many other cases illustrating the same point.

The Alkaline and Calcic Branches.

The two Scottish suites of Upper Palæozoic rocks just mentioned fall into opposite categories with reference to what is now becoming recognised as the most fundamental distinction to be made among igneous rocks. The earlier set is typical of the andesitic division and the later of the tephritic; or, using other equivalent names, the one belongs to the calcic (or "alkali-calcic") branch and the other to the alkaline. I will adopt the latter terminology as being generally familiar to petrologists; but the characteristics of the two branches, which are too well known to need recapitulation here, are more clearly definable in mineralogical than in chemical language. This twofold division of igneous rocks is, of course, in no wise a final or exhaustive treatment of the subject; but as a first step towards a natural or genetic classification it seems to be established beyond question. No third branch in any degree comparable with the two and distinct from them has been proposed. The charnockites and their allies represent but a single rock-series, and Rosenbusch has not made clear his reasons for separating them from the calcic rocks. The "spilitic" suite of Dewey and Flett is made to embrace a somewhat miscellaneous collection of types, and any close genetic relationship among them can scarcely be considered as proved. It is perhaps permissible to suggest that, *e.g.*, the quartz-diabases are, here as in Scotland, quite distinct in their affinities from the types rich in soda. These latter, constituting the bulk of the proposed suite, would seem to belong quite naturally to the alkaline branch, the question of the magmatic or solfataric origin of the albite being in this connection immaterial.

A given petrographical province is either of calcic or of alkaline facies, typical members of the two branches not being found together. The apparent exceptions are, I think, not such as to modify very seriously the general rule. Mr. Thomas, in describing an interesting suite of rocks from Western Pembrokeshire, recognises the alkaline affinities of most of them, but assigns some of the more basic types to the opposite branch. In a very varied assemblage we not infrequently meet with a few extreme types which, occurring in a calcic province, recall the characters of alkaline rocks, or conversely. Such anomalies have been pointed out by Daly, Whitman Cross, and others. They are found among the later derived types, referable to prolonged or repeated differentiation, and they are to be expected especially where the initial magma was not very strongly characterised as either calcic or alkaline.

Having regard to the known exposures of igneous rocks over the existing land-surface of the globe, it seems that there is a very decided preponderance of the calcic over the alkaline branch. This, as we shall see, is probably a fact of real significance, but it is nevertheless noticeable that increasing knowledge tends partly to redress the balance. In our own country, in addition to the Scottish Carboniferous rocks and those probably of Ordovician age in Pembrokeshire, we have the remarkable Lower Palæozoic intrusions of Assynt, in Sutherland, of strongly alkaline character, as

described by Dr. Teall and more recently by Dr. Shand; while Dr. Flett has recognised alkaline rocks of more than one age in Cornwall and Devon, and Mr. Tyrrell is engaged in studying another interesting province, of Permian age, in Ayrshire.

That the distinction between the alkaline and the calcic rocks embodies some principle of real and fundamental significance becomes very apparent when we look at the geographical distribution of the two branches. Taking what the German petrographers call the "younger" igneous rocks, *i.e.*, those belonging to the latest system of igneous activity, we find it possible to map out the active parts of the earth's crust into great continuous regions of alkaline rocks on the one hand and of calcic on the other. An alkaline region comprises numerous petrographical provinces, which may differ notably from one another, but agree in being all of alkaline facies. In like manner a common calcic facies unites other provinces, which collectively make up a continuous calcic region. Concerning the igneous rocks of earlier periods our knowledge is less complete, but, so far as it goes, it points to the same general conclusions.

These considerations enable us to simplify at the outset the problem before us. If we would seek the meaning and origin of petrographical provinces, we must inquire in the first place how igneous rocks as a whole come to group themselves under two great categories, which, at any one period of igneous activity, are found in separate regions of the earth's crust. The fact that a given district may form part of a calcic province at one period and of an alkaline one at another, precludes the hypothesis that the composition of igneous rocks depends in any degree upon peculiarities inherent from the beginning in the subjacent crust. The same objection applies with scarcely less force to various conflicting suggestions based on an assumed absorption or "assimilation" of sedimentary rocks by igneous magmas. Thus Jensen supposes the alkaline rocks to be derived by the assimilation or fusion of alkaline sediments at great depths. Daly propounds the more elaborate, and on a first view paradoxical, theory that alkaline have been derived from calcic magmas as a consequence of the absorption of limestone. These geologists agree in regarding the alkaline rocks as relatively unimportant in their actual development and in some sense abnormal in their origin. For Suess, on the other hand, it is the calcic rocks which owe their distinctive characters to an absorption of sedimentary material, enriching the magma in lime and magnesia. Apart from difficulties of the physical and chemical kind, all such theories fail to satisfy, in that they ignore the separation of the two branches of igneous rocks in different regions of the globe, each of which includes sediments of every kind. What then is the real significance of this regional separation? The obvious way of approaching the question is to inquire first whether the alkaline and calcic regions of the globe present any notable differences of a kind other than petrographical.

Relation between Tectonic and Petrographical Facies.

The close connection between igneous activity and displacements of the earth's crust has been traced by Suess, Lossen, Bertrand, de Lapparent, and others, and is a fact sufficiently well recognised. We have here, indeed, two different ways of relieving unequal stresses in the crust, and it is not surprising that they show a broad general coincidence both in space and in time. We can, however, go farther. Not only the distribution of igneous rocks in general, but the distribution of different kinds of rocks, is seen to stand in unmistakable relation to the leading tectonic features of the globe. It is very noticeable that petrographical provinces, and in particular provinces belonging to opposite branches, are often divided by important orographic lines. This is illustrated by the Cordilleran chain in both North and South America, and again by some of the principal arcs of the Alpine system in Europe. If, now, we examine the actual distribution more closely, in the light of Suess's analysis of the continents and oceanic basins, we perceive another relation still more significant. It is that, as regards the younger igneous rocks, the main alkaline and calcic regions correspond with the areas characterised by the Atlantic and Pacific types of coast-line respectively; I briefly drew attention to this correspondence in 1896, and a few years later Prof. Becke, of Vienna, arrived independently at the same generalisation. Recalling the two classes of crust-movements discriminated by Suess, he says it appears that the alkaline rocks are typically associated with subsidence due to radial contraction of the globe, and the calcic rocks with folding due to lateral compression. The greater part of Becke's memoir is devoted to a comparison of the two branches in respect of chemical composition; but here, I think, he has been misled by taking as representative of the whole alkaline "*Sippe*" or tribe the rocks of one small and peculiar province, that of the Bohemian Mittelgebirge. Some petrologists have followed Becke in adopting the terms Atlantic and Pacific as names, or at least synonyms, for the two branches of igneous rocks. Others, perhaps with some justice, deprecate the use of the same terms in a petrographical as well as a tectonic sense, so long as the implied relationship is still a matter of discussion.

I would point out in passing that the association of the alkaline rocks with areas of subsidence helps to explain the relatively small part which they play in the visible portion of the earth's surface. We may not unreasonably conjecture, for instance, that the volcanic islands scattered sparingly over the face of the Atlantic Ocean, from the Azores to Tristan d'Acunha, are merely fragments of a very extensive tract of alkaline rocks now submerged.

The generalisation associated with the name of Becke, in so far as it may ultimately commend itself to general acceptance, must have an important bearing on the problem of the origin of petrographical differences. The time is not ripe for any dogmatic pronouncement, but I will venture to indicate briefly the general trend of the inferences to be drawn. It seems clear that only a trivial effect at most can be allowed to original and permanent heterogeneity of the earth's crust, or to such accidents as the absorption by an igneous magma of a limited amount of the country-rock. The division between alkaline and calcic regions, and the separation of distinct provinces within such regions, point rather to the same general cause which, at a later stage, produced the diversity of rock-types within a single province, that is, to magmatic differentiation. Here, however, the differentiation postulated must be on a very wide scale, and must take effect in the horizontal direction. Its close connection with crust-movements clearly indicates differential stress as an essential element in the process. The actual mechanism can be at present only a matter of speculation, but I think the clue will be found in such observations as those of Mr. Barrow on the pegmatites of the Scottish Highlands. Conceive an extensive tract to be underlain by a zone which is neither solid nor liquid, but composed of crystals with an interstitial fluid magma. If this be subjected to different pressures in different parts of its horizontal extent, its uniformity will necessarily be disturbed, the fluid portion being squeezed out at places of higher pressure and driven to places of lower pressure. The precise nature of the differentiation thus set up will depend on the relative compositions of the crystalline and fluid portions, and the subject could not be very profitably discussed without fuller knowledge concerning the order of crystallisation in rock-magmas. Whether or not the explanation be ultimately found in this direction, the relation between the two tectonic types and the two branches of igneous rocks must, I think, find a place in the final solution of the problem.

I intimated at the outset that my remarks would not be confined to matters already settled and indisputable. It will be easily understood that some statements which I have made, for the sake of clearness, without qualification are subject to exceptions, and exceptions have, indeed, been urged by critics whose opinions are entitled to respect. The most uncompromising of these critics, Dr. Whitman Cross, has laid it down that: "Only generalisations without known exceptions in experience can be applied to the construction of a system that may be called natural." I hold, on the contrary, that such a science as Geology can be advanced only by the inductive method, which implies provisional hypotheses and successive approximations to the truth. A generalisation which brings together a mass of scattered observations, and endows them with meaning, is not invalidated by the discovery of exceptions. These

merely prove that it is not a final expression of the whole truth, and may point the way to its revision and correction.

Take, for instance, our provisional law of the distribution of the two branches of igneous rocks in defined regions. It has been objected that leucitic lavas, having therefore very decided alkaline or Atlantic affinities, are known at several places within the limits of the main Pacific region, where they are associated with andesitic and other calcic rocks. Now, the only area for which we have anything like full information is the island of Java. Here, according to Verbeek and Fennema, the great plateau-lavas of Tertiary age are exclusively of andesitic types, and the same is true of the long chain of 116 volcanic centres, which represent the later revival of activity. As against this record there are five volcanoes, long extinct, which at one stage erupted leucitic lavas. Whether we suppose these to be aberrant derivatives from an andesitic magma, or, much more probably, an incursion from the neighbouring alkaline region, it seems reasonable to regard these very exceptional occurrences as of the second order of importance, and to set them aside in a first attempt to reduce the facts to order.

The discovery of various alkaline rocks on Hawaii, Samoa, Raratonga, Tahiti, and other islands in the midst of the Pacific Ocean raises, I think, a different question. So far as is known, these rocks are not found in close association with characteristic calcic types. Suess's masterly discussion of all the geographical and hydrographical data hitherto obtained makes it clear that an Atlantic as well as a Pacific element of structure enters into some parts of the Pacific basin. In certain areas, such as the Galapagos Archipelago, the coming in of the Atlantic *régime* is quite clearly reflected in an alkaline facies of the igneous rocks, and such exceptions are therefore of the kind which go to prove the rule. Both Max Weber and Lacroix have expressed the opinion that the andesitic branch of rocks is characteristic of the border of the great Pacific basin rather than the interior. It is possible that further knowledge may justify this conclusion, and still only confirm the relation which is claimed between the two tectonic types and the two petrographical facies. Meanwhile, we find clear evidence elsewhere that vertical subsidence and lateral thrust have sometimes occurred in the same region or in the same petrographical province; nor need we go far from home to learn that the complexity of structure thus implied is accompanied by a corresponding peculiarity of petrographical facies.

The North British Tertiary Province.

In order to illustrate this point in a concrete instance, I will discuss very briefly a single petrographical province, viz. that which occupied the northern part of Britain in early Tertiary times. Prof. Judd has regarded this as forming part of a larger "Brito-Icelandic province"; but, while recognising many affinities between our rocks and those of higher latitudes, I think that the North British area possesses enough individuality to be more properly treated as a distinct unit. The record of igneous action here is exceptionally complete and well displayed. Our knowledge of it is derived, in the first place, from Prof. Zirkel, Sir Archibald Geikie, and Prof. Judd, and more recently from the detailed work carried out by the Geological Survey of Scotland. This latter is, as regards the Isle of Mull, still in progress, and will doubtless, when completed, throw additional light on some questions still obscure.

The province includes all western and southern Scotland, with the northern part of Ireland, and extends southward as far as Anglesey and Yorkshire; but the chief theatre of igneous activity was the sunken and faulted tract of the Inner Hebrides, between the mainland of Scotland, on the one hand, and the Archæan *massif* of the Outer Isles on the other. It is here that the volcanic accumulations attain their greatest thickness, and here, closely set along a N.-S. line, are the plutonic centres of Skye, Rum, Ardnamurchan, and Mull. Farther south are the volcanic plateau of Antrim and the neighbouring plutonic centres of the Mourne Mountains and Carlingford, while the two centres of Arran and that of Ailsa lie on a parallel line only a little farther east. In addition, it is clear that igneous activity extended westward over a tract now submerged under the Atlantic, and here, too, plutonic centres were not wanting. One is exposed in St. Kilda, 50 miles west of the Outer Hebrides, and another has been inferred by Prof. Cole from a study of the stones dredged on the Porcupine Bank, 150 miles west of Ireland.

The connection of igneous action in this province with the subsidence of faulted blocks of country is too plain to be missed; and so far, excepting the tendency to a definite alignment of the foci of activity, we seem to be dealing with a typical example of the Atlantic *régime*. The actual tectonic relations are, however, of a more complex kind, and undoubtedly involve the element of lateral thrust as well as vertical subsidence. This is more particularly in the neighbourhood of those special centres which were marked at one stage by plutonic intrusions. The evidence is seen in sharp anticlinal folding; sometimes also in crush-brecciation along quasi-horizontal bands and (in Rum) contemporaneous gneissic structure in the plutonic masses themselves. The disturbances in Mull, as described by Mr. Bailey, are especially interesting. The whole eastern coast-line of the island is determined by a system of concentric curved axes of folding, affecting all the rocks up to the Tertiary basalts, which are in places tilted almost vertically. The curved axes are disposed with reference to the plutonic centre of the island, and a somewhat similar arrangement is found on the east side of the Skye centre. All these facts go to show that in the district surrounding any one of the special centres there was developed a complex system of stresses, which found relief partly in igneous action, partly in displacements of the solid rocks. Nor were the effects confined to the plutonic phase. At a later epoch the influence of these local stresses is sometimes indicated by the diversion of the very numerous dykes from their normal north-westerly direction to a radial arrangement about the special centres, as is seen partly in Skye and more strikingly in Rum. There are also local groups of dykes developed only in these districts, and these again sometimes have a radial arrangement. More remarkable are the groups of inclined sheets which are found about the same centres, usually intersecting the plutonic rocks and a small fringing belt, and constantly dipping inwards. Such sheets occur in immense numbers in the gabbro mountains of Skye and Mull, and they are to be recognised also in Rum and Ardnamurchan.

It is plain, then, that this province exemplifies at once the two tectonic types distinguished by Suess. There has been a general subsidence affecting the area as a whole, but not all parts equally, and with this we must connect those groups of igneous rocks which have a wide distribution throughout the province. But there have also been movements in the lateral sense, more strictly localised and more sharply accentuated, and to these belong evidently the plutonic rocks with various other groups which are their satellites. I have pointed out these facts elsewhere, but failed to follow out the logical conclusion on the petrographical side. Influenced by the strongly marked characters of the plutonic series, I assigned the North British Tertiary rocks, not without some misgivings, to the calcic or Pacific region. Suess, having regard probably to the broader tectonic features rather than to petrographical data, has included our area in the Atlantic region.

Concerning the calcic facies of the plutonic rocks there can be no question. They constitute a well-defined "rock-series," intruded in order of decreasing basicity, and ranging from ultrabasic to thoroughly acid. The ultrabasic rocks, as developed in Rum and Skye, have a lime-felspar as one of their chief components: there are no picrites (in the original sense of Tschermak) or other alkaline types. The eucrite group, found in Rum, Ardnamurchan, and the Carlingford district, is also characterised by a felspar near anorthite. Gabbros are represented at nearly all the several centres, and in Arran they are accompanied by norites. The granites and granophyres fall into two subgroups. The less acid is usually augitic, while the more acid, found in Arran, St. Kilda, and the Mourne Mountains, carries hornblende and sometimes biotite.

This series is known in various provinces of Pacific facies. A peculiarity of it is that it is a broken series, types of mean acidity being absent. This has an interesting consequence. In many places a granite magma, in-

vading rocks so different from itself as gabbro or eucrite, has caused energetic mutual reactions, and a set of hybrid rocks has been produced, which serves in a limited sense to fill the gap in the series.

The only known exceptions to the calcic facies of our Tertiary plutonic rocks are perhaps significant in that they occur near the northern and southern limits of the principal belt of activity. The massive gently inclined sheets of granite and granophyre which make up part of the southern end of Raasay consist largely of microperthite, and contain abundant riebeckite, a distinctively alkaline mineral known at only one spot in Skye. The microperthitic granites of Arran do not carry riebeckite, but it is found in the well-known rock of Ailsa Craig, farther south.

The local groups of minor intrusions—acid, basic, and ultrabasic—related to the several plutonic centres have the same calcic facies as the plutonic rocks of which they are satellites. It appears, however, that they sometimes tend to a more alkaline composition towards the borders of their respective districts. Thus the Skye granite is surrounded by a roughly oval area, within which are found numerous dykes and sills of felsite and granophyre, in general augitic; but on the fringe of the area these rocks give place to orthophyres, with biotite or hornblende, and to bostonites.

Turn now to the rocks of regional distribution. The most important are, of course, the basalt lavas. They are all felspar-basalts, but a very general feature is the filling of their numerous amygdaloidal cavities with zeolites, such as analcime, natrolite, chabazite, and stilbite. These minerals are certainly not mere weathering-products. When I examined the basalts of Skye and the Small Isles some years ago, I regarded the zeolites as solfataric products, formed at the expense of the felspar by the action of volcanic water, while the rocks were still at a somewhat high temperature. Subsequent reconsideration has led me to regard these minerals rather as primary constituents of the rock, crystallised directly from the final residual magma, which had become relatively enriched in water by the abstraction of the anhydrous minerals. Such was the conclusion reached by Mr. James Strachan for the Antrim basalts, and a study of examples from Mull and Skye has enabled me to confirm and extend his interesting observations. Analcime in particular is not always confined to the steam-cavities, but in some cases occurs interstitially in the rock, where it is certainly not derived from felspar, and, indeed, has all the appearance of a primary constituent. The augite of these analcime-bearing basalts has in thin slices a purplish tint, with sensible pleochroism. From these and other features it appears that this group of rocks reveals on examination decided, though not very strongly marked, alkaline affinities.

Volcanic rocks of other than basaltic composition are not largely developed. They include both rhyolites and trachytes, the former without very distinctive characters, but the latter falling naturally into the alkaline division. In describing formerly a group of rhyolites and trachytes on the northern border of the Cuillins, I connected it with the neighbouring plutonic centre, but I have since found other trachytes in Skye: there is a fine development exposed in the glen above Bracadale. From this, and from the situation of the Antrim rhyolites, I infer that these felspathic and acid lavas, though distributed sporadically, belong to the regional or Atlantic suite.

Consider next the widespread group of basic sills. The common non-porphyritic dolerite sills have, in most districts, little that is indicative of alkaline affinities, though chemical analyses show a rather noteworthy amount of soda. In the porphyritic dolerites this characteristic is much more apparent, and, indeed, these rocks are almost identical with the "Markle type" so largely represented among the alkaline rocks of the Scottish Carboniferous province. Mugearite, a type still richer in alkalies, is likewise common to the two provinces. As we approach the limits of the principal belt of activity, alkaline characteristics become well marked even in the common non-porphyritic dolerites. This is shown in Raasay and the northern part of Skye by the coming in of the purple pleochroic augite, while farther north, in the Shiant Isles, analcime enters, and even, according to a record of Heddle, nepheline.[1] At the other extreme, in southern Arran, occur the analcime-dolerite sills of Clauchland and Dippin.

The regional basic dykes, which are mostly posterior in age to the sills, exhibit more variety of composition. Some with abundant porphyritic felspars resemble the Markle type of dolerite, and there are others of mugearitic nature, but these are only a minority. In Argyllshire there are basic dykes with purple pleochroic augite, and even some of camptonite and monchiquite; but these latter at least I should exclude as being probably of late Palæozoic age.[2] The undoubtedly Tertiary dykes, however, exhibit a variety which can be explained only as the result of repeated differentiation. The distribution of some of the groups indicates the existence at this late stage of subsidiary centres of differentiation, distinct from the plutonic centres. Thus trachyte dykes are found especially throughout a tract extending from the south-western part of Skye through the middle of Argyllshire, while there is an isolated area of these dykes about Drynoch, on the opposite side of the Skye mountains. Here we have an evidently alkaline type. On the other hand, there are rocks which, taken by themselves, must be assigned to the calcic division. Augite-andesites, for example, are well known, especially in parts of western Argyllshire, in Arran and the Cumbraes, and in the outlying districts of the north of Ireland, Anglesey, and the north-east of England. That these rocks have arisen as products of a subsidiary differentiation we have in some cases almost ocular demonstration; for in Arran and elsewhere augite-andesites are found in remarkably intimate association with complementary types, often pitchstones of alkaline composition.

Even from so brief and imperfect a sketch we may, I think, draw some conclusions which have a wider application. This province exemplifies at once the two main tectonic types, and also comprises representatives of the two great branches of igneous rocks. Those rocks which are related to broad movements of Atlantic type indicate a parent magma of decided, though not strongly marked, alkaline nature; while those related to local movements of Pacific type clearly come from a calcic magma. There are some facts which suggest that the rocks tend to become more alkaline as we recede from the chief centres of activity, and this suggestion applies to some calcic as well as alkaline groups of rocks. Finally, it appears that the relative simplicity of arrangement was disturbed at a late stage by the effects of subsidiary differentiation, the province tending then to break up into districts related to new centres. Operating upon an initial magma not very strongly characterised, this later differentiation has even given rise to aberrant rock-types which overstep the petrographical boundary-line between the two branches.

Petrogenesis and Systematic Petrography.

From such considerations as I have hastily passed in review, it is evident that a survey of igneous rocks as they actually occur in the field leads to a conception of their mutual relationships very different from that embodied in the current schemes of systematic petrography. It may be of some interest, in conclusion, to expand this remark a little farther, although I am sensible that in so doing I lay myself open to the charge of vain speculation.

From the petrogenetic point of view, the most fundamental division among igneous rocks is that between the alkaline and calcic branches. This result, independently arrived at on petrographical grounds by several authorities, seems to be firmly established by the broad distribution of the two branches in different regions of the globe. But, if this argument be admitted, it follows that the next step in a natural grouping of igneous rocks should be suggested by a comparison of the characteristics of the various provinces into which the great regions divide. Many of these provinces have now been partly studied, and their special characteristics can often be expressed in concise terms: *e.g.* among alkaline rocks the relative proportion of potash to soda may be a characteristic common to a whole province. More precisely, by averaging the

[1] The dolerite here is intimately associated with ultrabasic rocks, as has been described by Judd.

[2] A like remark applies to the highly alkaline dykes of the Orkneys, which do not agree even in direction with the Tertiary suite.

chemical analyses of the chief rock-types, weighted according to their relative abundance, it is possible to calculate approximately the composition of the parent-magma of a province. Noting that nearly identical assemblages of rocks sometimes occur in widely separate provinces and at different geological periods, we have some reason for expecting that the provincial parent-magmas may ultimately be reduced to a limited number of types. Whether these types will be sufficiently definite to serve as a basis of classification it is too early to say.

For the sake of argument, I have taken chemical composition as the criterion. It is certain, however, that a rock-magma consists, not of free oxides, but mainly of silicate-compounds, and the variation produced by magmatic differentiation is a variation in the relative proportions of such compounds. The characteristics common to a set of cognate rock-types will therefore be more properly expressed in mineralogical than in chemical terms. If, to fix ideas, we take as representative of a province its principal plutonic series, we shall often find that some particular mineral or some special association of minerals stands out as a distinctive feature. For instance, in the charnockite-norite series of southern India the characteristic ferro-magnesian mineral is hypersthene; in the granite-gabbro series of the British Tertiary it is augite; and in the granite-diorite series, which predominates among the "newer granites" of the Scottish Highlands, hornblende and biotite. These three sets of rocks, all of calcic facies, are easily distinguishable in isolated specimens.

Each such rock-series embraces types ranging from acid to ultrabasic. This variation is ascribed to a later differentiation of the parent-magma of the province, and, therefore, in an arrangement based on genetic principles, it will find expression, not in the main divisions of the scheme, but in the subdivisions. Here is an essential difference between an ideal petrogenetic classification and the petrographical systems which are, or have been, in use. If we are content to limit our study of igneous rocks to specimens in a museum, the distinction of acid, neutral, basic, and ultrabasic may seem to be one of first importance. It has, in fact, been employed for the primary divisions in some formal schemes, *e.g.* in that put forward by Löwinson-Lessing. In a less crude system, like that of Rosenbusch, this element disappears, but the underlying idea still remains. There is a division into families, such as the granite-family and the gabbro-family, but the term, in so far as it implies blood-relationship, is a misnomer. The augite-granite of Mull is evidently more closely related to its associated gabbro than it is, say, to the biotite-granite of Peterhead or the hypersthene-granite of Madras.

The differentiation which evolves a varied series of plutonic rocks from a common parent-magma is clearly not of the same kind as that which gave rise to the parent-magma itself. It appears that the external mechanical element is here a less important factor, and the variation set up is, therefore, more closely in accordance with the uninterrupted course of crystallisation. This is clearly indicated when we compare the order of intrusion of the several rocks of the series with the order of crystallisation of their constituent minerals. The history of the series is in a sense epitomised in the history of each individual type, corresponding in both cases with continued fall of temperature and progressive change in the composition of the residual magma. In a large number of rocks, more particularly those of complex constitution, the order of crystallisation follows Rosenbusch's empirical law of decreasing basicity, and the plutonic intrusions then begin with the most basic type and end with the most acid. I mention this only to point out that, while the larger divisions of our ideal classification will have a certain geographical and tectonic significance, the subdivisions will show a certain correspondence with the sequence in time of the various cognate rock-types.

To pursue the subject farther would serve no useful purpose. It is clear that, if a natural—by which I mean a genetic—classification of igneous rocks is ever to become a reality, much work must first be done, both in the field and in the laboratory, each petrographical province being studied from the definite standpoint of the evolution of its rock-types from one parent stock. Such researches as those of Brögger in the Christiania province may serve as a model. It would be rash to venture at present more than the most general forecast of the lines which future developments may follow; but I think it calls for no less hardihood to set limits to what may ultimately be possible in this direction. There are those who would have us abandon in despair all endeavour to place petrography upon a genetic basis, and fall back upon a rigid arbitrary system as a final solution of the difficulty. This would be to renounce for ever the claim of this branch of geology to rank as a rational science. I have said enough to show that I am one of those who take a more hopeful view of the future of petrology, confidently expecting it to show, like the past, a record of continued progress.

SECTION D.

ZOOLOGY.

OPENING ADDRESS BY PROF. D'ARCY WENTWORTH THOMPSON, C.B., PRESIDENT OF THE SECTION.

Magnalia Naturae; or, The Greater Problems of Biology.

THE science of Zoology, all the more the incorporate science of Biology, is no simple affair, and from its earliest beginnings it has been a great and complex and many-sided thing. We can scarce get a broader view of it than from Aristotle, for no man has ever looked upon our science with a more far-seeing and comprehending eye. Aristotle was all things that we mean by "naturalist" or "biologist." He was a student of the ways and doings of beast and bird and creeping thing; he was morphologist and embryologist; he had the keenest insight into physiological problems, though lacking that knowledge of the physical sciences without which physiology can go but a little way: he was the first and is the greatest of psychologists; and in the light of his genius biology merged in a great philosophy.

I do not for a moment suppose that the vast multitude of facts which Aristotle records were all, or even mostly, the fruit of his own immediate and independent observation. Before him were the Hippocratic and other schools of physicians and anatomists. Before him there were nameless and forgotten Fabres, Röesels, Réaumurs, and Hubers, who observed the habits, the diet, and the habitations of the sand-wasp or the mason-bee; who traced out the little lives, and discerned the vocal organs, of grasshopper and cicada; and who, together with generations of bee-keeping peasants, gathered up the lore and wisdom of the bee. There were fishermen skilled in all the cunning of their craft, who discussed the wanderings of tunny and mackerel, sword-fish or anchovy; who argued over the ages, the breeding-places, and the food of this fish or that; who knew how the smooth dogfish breeds two thousand years before Johannes Müller; who saw how the male pipe-fish carries its young before Cavolini; and who had found the nest of the nest-building rock-fishes before Gerbe rediscovered it almost in our own day. There were curious students of the cuttle-fish (I sometimes imagine they may have been priests of that sea-born goddess to whom the creatures were sacred) who had diagnosed the species, recorded the habits, and dissected the anatomy of the group, even to the discovery of that strange hectocotylus arm that baffled Della Chiaje, Cuvier, and Koelliker, and that Verany and Heinrich Müller re-explained.

All this varied learning Aristotle gathered up and wove into his great web. But every here and there, in words that are unmistakably the master's own, we hear him speak of what are still the great problems and even the hidden mysteries of our science; of such things as the nature of variation, of the struggle for existence, of specific and generic differentiation of form, of the origin of the tissues, the problems of heredity, the mystery of sex, of the phenomena of reproduction and growth, the characteristics of habit, instinct, and intelligence, and of the very meaning of Life itself. Amid all the maze of concrete facts that century after century keeps adding to our store, these, and such as these, remain the great mysteries of natural science—the *Magnalia naturae*, to borrow a great word from Bacon, who in his turn had borrowed it from St. Paul.

Not that these are the only great problems for the biologist, nor that there is even but a single class of great problems in Biology. For Bacon himself speaks of the *magnalia naturae, quoad usus humanos*, the study of which has for its objects "the prolongation of life or the retardation of age, the curing of diseases counted incurable, the mitigation of pain, the making of new species and transplanting of one species into another," and so on through many more. Assuredly I have no need to remind you that a great feature of this generation of ours has been the way in which Biology has been justified of her children, in the work of those who have studied the *magnalia naturae, quoad usus humanos*.

But so far are biologists from being nowadays engrossed in practical questions, in applied and technical Zoology, to the neglect of its more recondite problems, that there never was a time when men thought more deeply or laboured with greater zeal over the fundamental phenomena of living things; never a time when they reflected in a broader spirit over such questions as purposive adaptation, the harmonious working of the fabric of the body in relation to environment, and the interplay of all the creatures that people the earth; over the problems of heredity and variation; over the mysteries of sex, and the phenomena of generation and reproduction, by which phenomena, as the wise woman told, or reminded, Socrates, and as Harvey said again (and for that matter, as Coleridge said, and Weismann, but not quite so well)—by which, as the wise old woman said, we gain our glimpse of insight into eternity and immortality. These, then, together with the problem of the Origin of Species, are indeed *magnalia naturae*; and I take it that inquiry into these, deep and wide research specially directed to the solution of these, is characteristic of the spirit of our time, and is the pass-word of the younger generation of biologists.

Interwoven with this high aim which is manifested in the biological work of recent years is another tendency. It is the desire to bring to bear upon our science, in greater measure than before, the methods and results of the other sciences, both those that in the hierarchy of knowledge are set above and below, and those that rank alongside of our own.

Before the great problems of which I have spoken, the cleft between Zoology and Botany fades away, for the same problems are common to the twin sciences. When the zoologist becomes a student, not of the dead, but of the living, of the vital processes of the cell rather than of the dry bones of the body, he becomes once more a physiologist, and the gulf between these two disciplines disappears. When he becomes a physiologist, he becomes, *ipso facto*, a student of chemistry and of physics. Even mathematics has been pressed into the service of the biologist, and the calculus of probabilities is not the only branch of mathematics to which he may usefully appeal.

The physiologist has long had as his distinguishing characteristic, giving his craft a rank superior to the sister branch of morphology, the fact that in his great field of work, and in all the routine of his experimental research, the methods of the physicist and the chemist, the lessons of the anatomist, and the experience of the physician are inextricably blended in one common central field of investigation and thought. But it is much more recently that the morphologist and embryologist have made use of the method of experiment, and of the aid of the physical and chemical sciences—even of the teachings of philosophy: all in order to probe into properties of the living organism that men were wont to take for granted, or to regard as beyond their reach, under a narrower interpretation of the business of the biologist. Driesch and Loeb and Roux are three among many men who have become eminent in this way in recent years, and their work we may take as typical of methods and aims such as those of which I speak. Driesch, both by careful experiment and by philosophic insight, Loeb, by his conception of the dynamics of the cell and by his marvellous demonstrations of chemical and mechanical fertilisation, Roux, with his theory of autodetermination, and by all the labours of the school of *Entwickelungsmechanik* which he has founded, have all in various ways, and from more or less different points of view, helped to reconstruct and readjust our ideas of the relations of embryological processes, and hence of the phenomenon of life itself, on the one hand to physical causes (whether external to or latent in the mechanism of the cell), or on the other to the ancient conception of a Vital Element alien to the province of the physicist.

No small number of theories or hypotheses, that seemed for a time to have been established on ground as firm as that on which we tread, have been reopened in our day. The adequacy of natural selection to explain the whole of organic evolution has been assailed on many sides; the old fundamental subject of embryological debate between the evolutionists or preformationists (of the school of Malpighi, Haller, and Bonnet) and the advocates of epigenesis (the followers of Aristotle, of Harvey, of Caspar F. Wolff, and of Von Baer) is now discussed again, in altered language, but as a pressing question of the hour; the very foundations of the cell-theory have been scrutinised to decide (for instance) whether the segmented ovum, or even the complete organism, be a colony of quasi-independent cells, or a living unit in which cell differentiation is little more than a superficial phenomenon; the whole meaning, bearing, and philosophy of evolution has been discussed by Bergson, on a plane to which neither Darwin nor Spencer ever attained; and the hypothesis of a Vital Principle, or vital element, that had lain in the background for near a hundred years, has come into men's mouths as a very real and urgent question, the greatest question for the biologist of all.

In all ages the mystery of organic form, the mystery of growth and reproduction, the mystery of thought and consciousness, the whole mystery of the complex phenomena of life, have seemed to the vast majority of men to call for description and explanation in terms alien to the language which we apply to inanimate things; though at all times there have been a few who sought, with the materialism of Democritus, Lucretius, or Giordano Bruno, to attribute most, or even all, of these phenomena to the category of physical causation.

For the first scientific exposition of Vitalism we must go back to Aristotle, and to his doctrine of the three parts of the tripartite Soul: according to which doctrine, in Milton's language, created things "by gradual change sublimed, To vital spirits aspire, to animal, To intellectual!" The first and lowest of these three, the ψυχὴ ἡ θρεπτική, by whose agency nutrition is effected, is ἡ πρώτη ψυχή, the inseparable concomitant of Life itself. It is inherent in the plant as well as in the animal, and in the Linnæan aphorism, *Vegetabilia crescunt et vivunt*, its existence is admitted in a word. Under other aspects, it is all but identical with the ψυχὴ αὐξητική and γεννητική, the soul of growth and of reproduction: and in this composite sense it is no other than Driesch's "Entelechy," the hypothetic natural agency that presides over the form and formation of the body. Just as Driesch's psychoid or psychoids, which are the basis of instinctive phenomena, of sensation, instinct, thought, reason, and all that directs that body which entelechy has formed, are no other than the αἰσθητική whereby *animalia vivunt et sentiunt*, and the διανοητική to which Aristotle ascribes the reasoning faculty of man. Save only that Driesch, like Darwin, would deny the restriction of νοῦς, or reasoning, to man alone, and would extend it to animals, it is clear, and Driesch himself admits,[1] that he accepts both the vitalism and the analysis of vitalism laid down by Aristotle.

The πνεῦμα of Galen, the *vis plastica*, the *vis vitae formatrix*, of the older physiologists, the *Bildungstrieb* of Blumenbach, the *Lebenskraft* of Paracelsus, Stahl, and Treviranus, "shaping the physical forces of the body to its own ends," "dreaming dimly in the grain of the promise of the full corn in the ear,"[2] these and many more, like Driesch's "entelechy" of to-day, are all conceptions under which successive generations strive to depict

[1] "Science and Philosophy of the Organism" (Gifford Lectures), ii. p. 83, 1908.

[2] *Cit.* Jenkinson (*Art.* "Vitalism" in *Hibbert Journal*, April, 1911) who has given me the following quotation: "Das Weitzenkorn hat allerdings Bewusstsein dessen was in ihm ist und aus ihm werden kann, und träümt wirklich davon. Sein Bewusstsein und seine Träume mögen dunkel genug sein"; Treviranus, "Erscheinungen und Gesetze des organischen Lebens, 831.

the something that separates the earthy from the living, the living from the dead. And John Hunter described his conception of it in words not very different from Driesch's, when he said that his principle, or agent, was independent of organisation, which yet it animates, sustains, and repairs; it was the same as Johannes Müller's conception of an innate "unconscious idea."

Even in the Middle Ages, long before Descartes, we can trace, if we interpret the language and the spirit of the time, an antithesis that, if not identical, is at least parallel to our alternative between vitalistic and mechanical hypotheses. For instance, Father Harper tells us that Suarez maintained, in opposition to St. Thomas, that in generation and development a Divine interference is postulated, by reason of the perfection of living beings; in opposition to St. Thomas, who (while invariably making an exception in the case of the human soul) urged that, since the existence of bodily and natural forms consists solely in their union with matter, the ordinary agencies which operate on matter sufficiently account for them.[1]

But in the history of modern science, or of modern physiology, it is, of course, to Descartes that we trace the origin of our mechanical hypotheses—to Descartes, who, imitating Archimedes, said, "Give me matter and motion, and I will construct the universe." In fact, leaving the more shadowy past alone, we may say that it is since Descartes watched the fountains in the garden, and saw the likeness between their machinery of pumps and pipes and reservoirs and the organs of the circulation of the blood, and since Vaucanson's marvellous automata lent plausibility to the idea of a "living automaton"—it is since then that men's minds have been perpetually swayed by one or other of the two conflicting tendencies, either to seek an explanation of the phenomena of living things in physical and mechanical considerations, or to attribute them to unknown and mysterious causes, alien to physics and peculiarly concomitant with life. And some men's temperaments, training, and even avocations, render them more prone to the one side of this unending controversy, as the minds of other men are naturally more open to the other. As Kühne said a few years ago at Cambridge, the physiologists have been found for several generations leaning, on the whole, to the mechanical or physico-chemical hypothesis, while the zoologists have been very generally on the side of the Vitalists.

The very fact that the physiologists were trained in the school of physics, and the fact that the zoologists and botanists relied for so many years upon the vague, undefined force of "heredity" as sufficiently accounting for the development of the organism, an intrinsic force the results of which could be studied, but the nature of which seemed remote from possible analysis or explanation—these facts alone go far to illustrate and to justify what Kühne said.

Claude Bernard held that mechanical, physical, and chemical forces summed up all with which the physiologist has to deal. Verworn defined physiology as "the chemistry of the proteids"; and I think that another physiologist (but I forget who) has declared that the mystery of life lay hidden in "the chemistry of the enzymes." But of late, as Dr. Haldane showed in his address a couple of years ago to the Physiological Section, it is among the physiologists themselves, together with the embryologists, that we find the strongest indications of a desire to pass beyond the horizon of Descartes, and to avow that physical and chemical methods, the methods of Helmholtz, Ludwig, and Claude Bernard, fall short of solving the secrets of physiology. On the other hand, in zoology, resort to the method of experiment—the discovery, for instance, of the wonderful effects of chemical or even mechanical stimulation in starting the development of the egg—and again the ceaseless search into the minute structure, or so-called mechanism, of the cell—these, I think, have rather tended to sway a certain number of zoologists in the direction of the mechanical hypothesis.

But, on the whole, I think it is very manifest that there is abroad on all sides a greater spirit of hesitation and caution than of old, and that the lessons of the philosopher have had their influence on our minds. We realise that the problem of development is far harder than we had begun to let ourselves suppose: that the problems of organogeny and phylogeny (as well as those of physiology) are not comparatively simple and well-nigh solved, but are of the most formidable complexity. And we would, most of us, confess, with the learned author of "The Cell in Development and Inheritance," "that we are utterly ignorant of the manner in which the substance of the germ-cell can so respond to the influence of the environment as to call forth an adaptive variation; and again, that the gulf between the lowest forms of life and the inorganic world is as wide as, if not wider than, it seemed a couple of generations ago."[1]

While we keep an open mind on this question of Vitalism, or while we lean, as so many of us now do, or even cling with a great yearning, to the belief that something other than the physical forces animates and sustains the dust of which we are made, it is rather the business of the philosopher than of the biologist, or of the biologist only, when he has served his humble and severe apprenticeship to philosophy, to deal with the ultimate problem. It is the plain bounden duty of the biologist to pursue his course, unprejudiced by vitalistic hypotheses, along the road of observation and experiment, according to the accepted discipline of the natural and physical sciences; indeed, I might perhaps better say the physical sciences alone, for it is already a breach of their discipline to invoke, until we feel we absolutely must, that shadowy force of "heredity" to which, as I have already said, biologists have been accustomed to ascribe so much. In other words, it is an elementary scientific duty; it is a rule that Kant himself laid down[2] that we should explain, just as far as we possibly can, all that is capable of such explanation in the light of the properties of matter and of the forms of energy with which we are already acquainted.

It is of the essence of physiological science to investigate the manifestations of energy in the body, and to refer them, for instance, to the domains of heat, electricity, or chemical activity. By this means a vast number of phenomena, of chemical and other actions of the body, have been relegated to the domain of physical science and withdrawn from the mystery that still attends on life: and by this means, continued for generations, the physiologists, or certain of them, now tell us that we begin again to descry the limitations of physical inquiry, and the region where a very different hypothesis insists on thrusting itself in. But the morphologist has not gone nearly so far as the physiologist in the use of physical methods. He sees so great a gulf between the crystal and the cell, that the very fact of the physicist and the mathematician being able to explain the form of the one, by simple laws of spatial arrangement where molecule fits into molecule, seems to deter, rather than to attract, the biologist from attempting to explain organic forms by mathematical or physical law. Just as the embryologist used to explain everything by heredity, so the morphologist is still inclined to say—"the thing is alive, its form is an attribute of itself, and the physical forces do not apply." If he does not go so far as this, he is still apt to take it for granted that the physical forces can only to a small and even insignificant extent blend with the intrinsic organic forces in producing the resultant form. Herein lies our question in a nutshell. Has the morphologist yet sufficiently studied the forms, external and internal, of organisms in the light of the properties of matter, of the energies that are associated with it, and of the forces by which the actions of these energies may be interpreted and described? Has the biologist, in short, fully recognised that there is a borderland, not only between physiology and physics, but between morphology and physics, and that the physicist may, and must, be his guide and teacher in many matters regarding organic form?

Now this is by no means a new subject, for such men as Berthold and Errera, Rhumbler and Dreyer, Bütschli and Verworn, Driesch and Roux, have already dealt or

[1] "Cum formarum naturalium et corporalium esse non consistat nisi in unione ad materiam; ejusdem agentis esse videtur eas producere, cujus est materiam transmutare. Secundo, quia cum hujusmodi formae non excedant virtutem et ordinem et facultatem principiorum agentium in natura, nulla videtur necessitas eorum originem in principia reducere altiora.—Aquinas, *De Pot.* Q. iii., a, 11: cf. Harper, "Metaphysics of the School," iii. 1, p. 152.

[1] Wilson, *op. cit.* 1906, p. 434. [2] In his "Critique of Teleological Judgment."

deal with it. But on the whole it seems to me that the subject has attracted too little attention, and that it is well worth our while to think of it to-day.

The first point, then, that I wish to make in this connection is that the Form of any portion of matter, whether it be living or dead, its form and the changes of form that are apparent in its movements and in its growth, may in all cases alike be described as due to the action of Force. In short, the form of an object is a "diagram of forces"—in this sense, at least, that from it we can judge of or deduce the forces that are acting or have acted upon it; in this strict and particular sense it is a diagram; in the case of a solid, of the forces that *have* been impressed upon it when its conformation was produced, together with those that enable it to retain its conformation; in the case of a liquid (or of a gas), of the forces that are for the moment acting on it to restrain or balance its own inherent mobility. In an organism, great or small, it is not merely the nature of the *motions* of the living substance that we must interpret in terms of Force (according to Kinetics), but also the *conformation* of the organism itself, whose permanence or equilibrium is explained by the interaction or balance of forces, as described in Statics.

If we look at the living cell of an Amœba or a Spirogyra we see a something which exhibits certain active movements, and a certain fluctuating, or more or less lasting, form; and its form at a given moment, just like its motions, is to be investigated by the help of physical methods, and explained by the invocation of the mathematical conception of force.

Now the state, including the shape or form, of a portion of matter is the resultant of a number of forces, which represent or symbolise the manifestations of various kinds of Energy; and it is obvious, accordingly, that a great part of physical science must be understood or taken for granted as the necessary preliminary to the discussion on which we are engaged.

I am not going to attempt to deal with, or even to enumerate, all the physical forces or the properties of matter with which the pursuit of this subject would oblige us to deal—with gravity, pressure, cohesion, friction, viscosity, elasticity, diffusion, and all the rest of the physical factors that have a bearing on our problem. I propose only to take one or two illustrations from the subject of *surface-tension*, which subject has already so largely engaged the attention of the physiologists. Nor will I even attempt to sketch the general nature of this phenomenon, but will only state (as I fear for my purpose I must) a few of its physical manifestations or laws. Of these, the most essential facts for us are as follows:—Surface-tension is manifested only in fluid or semi-fluid bodies, and only at the surface of these: though we may have to interpret surface in a liberal sense in cases where the interior of the mass is other than homogeneous. Secondly, a fluid may, according to the nature of the substance with which it is in contact, or (more strictly speaking) according to the distribution of energy in the system to which it belongs, tend either to spread itself out in a film, or, conversely, to contract into a drop, striving in the latter case to reduce its surface to a minimal area. Thirdly, when three substances are in contact (and subject to surface-tension), as when water surrounds a drop of protoplasm in contact with a solid, then at any and every point of contact certain definite angles of equilibrium are set up and maintained between the three bodies, which angles are proportionate to the magnitudes of the surface-tensions existing between the three. Fourthly, a fluid film can only remain in equilibrium when its curvature is everywhere constant. Fifthly, the only surfaces of revolution which meet this condition are six in number, of which the plane, the sphere, the cylinder, and the so-called unduloid and catenoid are the most important. Sixthly, the cylinder cannot remain in free equilibrium if prolonged beyond a length equal to its own circumference, but, passing through the unduloid, tends to break up into spheres: though this limitation may be counteracted or relaxed, for instance, by viscosity. Finally, we have the curious fact that, in a complex system of films, such as a homogeneous froth of bubbles, three partition-walls and no more always meet at a crest, at equal angles, as, for instance, in the very simple case of a layer of uniform hexagonal cells; and (in a solid system) the crests, which may be straight or curved, always meet, also at equal angles, four by four, in a common point. From these physical facts, or laws, the morphologist, as well as the physiologist, may draw important consequences.

It was Hofmeister who first showed, more than forty years ago, that when any drop of protoplasm, either over all its surface or at some free end (as at the tip of the pseudopodium of an Amœba), is seen to "round itself off," that is not the effect of physiological or vital contractility, but is a simple consequence of surface-tension—of the law of the minimal surface; and in the physiological side, Engelmann, Bütschli, and others have gone far in their development of the idea.

It was Plateau, I think, who first showed that the myriad sticky drops or beads upon the weft of a spider's web, their form, their size, their distance apart, and the presence of the tiny intermediate drops between, were in every detail explicable as the result of surface-tension, through the law of minimal surface and through the corollary to it which defines the limits of stability of the cylinder; and, accordingly, that with their production, the will or effort or intelligence of the spider had nothing to do. The beaded form of a long, thin pseudopodium, for instance, of a Heliozoan, is an identical phenomenon.

It was Errera who first conceived the idea that not only the naked surface of the cell, but the contiguous surfaces of two naked cells, or the delicate incipient cell-membrane or cell-wall between, might be regarded as a weightless film, whose position and form were assumed in obedience to surface-tension. And it was he who first showed that the symmetrical forms of the unicellular and simple multicellular organisms, up to the point where the development of a skeleton complicates the case, were one and all identical with the plane, sphere, cylinder, unduloid and catenoid, or with combinations of these.

It was Berthold and Errera who, almost simultaneously, showed (the former in far the greater detail) that in a plant each new cell-partition follows the law of minimal surface, and tends (according to another law which I have not particularised) to set itself at right angles to the preceding solidified wall, so giving a simple and adequate physical explanation of what Sachs had stated as an empirical morphological rule. And Berthold further showed how, when the cell-partition was curved, its precise curvature, as well as its position, was in accordance with physical law.

There are a vast number of other things that we can satisfactorily explain on the same principle and by the same laws. The beautiful catenary curve of the edge of the pseudopodium, as it creeps up its axial rod in a Heliozoan or a Radiolarian, the hexagonal mesh of bubbles, or vacuoles, on the surface of the same creatures, the form of the little groove that runs round the waist of a Peridinian, even (as I believe) the existence, form, and undulatory movements of the undulatory membrane of a Trypanosome, or of that around the tail of the spermatozoon of a newt—every one of these, I declare, is a case where the resultant form can be well explained by, and cannot possibly be understood without, the phenomena of surface-tension; indeed, in many of the simpler cases the facts are so well explained by surface-tension that it is difficult to find place for a conflicting, much less an overriding, force.

I believe, for my own part, that even the beautiful and varied forms of the Foraminifera may be ascribed to the same cause; but here the problem is just a little more complex, by reason of the successive consolidations of the shell. Suppose the first cell or chamber to be formed, assuming its globular shape in obedience to our law, and then to secrete its calcareous envelope. The new growing bud of protoplasm, accumulating outside the shell, will, in strict accordance with the surface-tensions concerned, either fail to "wet" or to adhere to the first-formed shell, and will so detach itself as a unicellular individual (Orbulina), or else it will flow over a less or greater part of the original shell until its free surface meets it at the required angle of equilibrium. Then, according to this angle, the second chamber may happen to be all but detached

(Globigerina), or, with all intermediate degrees, may very nearly wholly enwrap the first. Take any specific angle of contact and presume the same conditions to be maintained, and therefore the same angle to be repeated, as each successive chamber follows on the one before, and you will thereby build up regular forms, spiral or alternate, that correspond with marvellous accuracy to the actual forms of the Foraminifera. And this case is all the more interesting, because the allied and successive forms so obtained differ only in degree in the magnitude of a single physical or mathematical factor; in other words, we get not only individual phenomena, but lines of apparent *orthogenesis*, that seem explicable by physical laws, and attributable to the continuity between successive states in the continuous or gradual variation of a physical condition. The resemblance between allied and related forms, as Hartmann demonstrated and Giard admitted years ago, is not always, however often, to be explained by common descent and parentage.[1]

In the segmenting egg we have the simpler phenomenon of a "laminar system," uncomplicated by the presence of a solid framework; and here, in the earliest stages of segmentation, it is easy to see the correspondence of the planes of division with what the laws of surface-tension demand. For instance, it is not the case (though the elementary books often represent it so) that when the totally segmenting egg has divided into four segments, the four partition walls ever remain in contact at a single point; the arrangement would be unstable, and the position untenable. But the laws of surface-tension are at once seen to be obeyed when we recognise the little *cross-furrow* that separates the blastomeres, two and two, leaving in each case three only to meet at a point in our diagram, which point is in reality a section of a ridge or crest.

Very few have tried, and one or two (I know) have tried and not succeeded, to trace the action and the effects of surface-tension in the case of a highly complicated, multi-segmented egg. But it is not surprising if the difficulties which such a case presents appear to be formidable. Even the conformation of the interior of a soap-froth, though absolutely conditioned by surface-tension, presents great difficulties, and it was only in the last years of Lord Kelvin's life that he showed all previous workers to have been in error regarding the form of the interior cells.

But what, for us, does all this amount to? It at least suggests the possibility of so far supporting the observed facts of organic form on mathematical principles as to bring morphology within or very near to Kant's demand that a true natural science should be justified by its relation to mathematics.[2] But if we were to carry these principles further, and to succeed in proving them applicable in detail, even to the showing that the manifold segmentation of the egg was but an exquisite froth, would it wholly revolutionise our biological ideas? It would greatly modify some of them, and some of the most cherished ideas of the majority of embryologists; but I think that the way is already paved for some such modification. When Loeb and others have shown us that half, or even a small portion, of an egg, or a single one of its many blastomeres, can give rise to an entire embryo, and that in some cases *any* part of the ovum can originate *any* part of the organism, surely our eyes are turned to the *energies* inherent in the matter of the egg (not to speak of a presiding entelechy), and away from its original formal operations of division. Sedgwick has told us for many years that we look too much to the individuality of the individual cell, and that the organism, at least in the embryonic body, is a continuous syncytium. Hofmeister and Sachs have repeatedly told us that in the plant, the growth of the mass, the growth of the organ, is the primary fact; and De Bary has summed up the matter in his aphorism, *Die Pflanze bildet Zellen, nicht die Zelle bildet Pflanzen.* And in many other ways, as many of you are well aware, the extreme position of the cell-theory, that the cells are the ultimate individuals, and that the organism is but a colony of quasi-independent cells, has of late years been called in question.

There are no problems connected with Morphology that appeal so closely to my mind, or to my temperament, as those that are related to mechanical considerations, to mathematical laws, or to physical and chemical processes.

I love to think of the logarithmic spiral that is engraven over the grave of that great anatomist, John Goodsir (as it was over that of the greatest of the Bernouillis), so graven because it interprets the form of every molluscan shell, of tusk and horn and claw, and many another organic form besides. I like to dwell upon those lines of mechanical stress and strain in a bone that give it its strength where strength is required, that Hermann Meyer and J. Wolff described, and on which Roux has bestowed some of his most thoughtful work; or on the "stream-lines" in the bodily form of fish or bird, from which the naval architect and the aviator have learned so much. I admire that old paper of Peter Harting's in which he paved the way for investigation of the origin of spicules, and of all the questions of crystallisation or pseudo-crystallisation in presence of colloids, on which subject Lehmann has written his recent and beautiful book. I sympathise with the efforts of Henking, Rhumbler, Hartog, Gallardo, Leduc, and others to explain on physical lines the phenomena of nuclear division. And, as I have said to-day, I believe that the forces of surface-tension, elasticity, and pressure are adequate to account for a great multitude of the simpler phenomena, and the permutations and combinations thereof, that are illustrated in organic Form.

I should gladly and easily have spent all my time this morning in dealing with these questions alone. But I was loath to do so, lest I should seem to overrate their importance, and to appear to you as an advocate of a purely mechanical biology.

I believe all these phenomena to have been unduly neglected, and to call for more attention than they have received. But I know well that though we push such explanations to the uttermost, and learn much in the so doing, they will not touch the heart of the great problems that lie deeper than the physical plane. Over the ultimate problems and causes of vitality, over what is implied in the organisation of the living organism, we shall be left wondering still.

To a man of letters and the world like Addison, it came as a sort of revelation that Light and Colour were not objective things, but subjective, and that back of them lay only motion or vibration, some simple activity. And when he wrote his essay on these startling discoveries, he found for it, from Ovid, a motto well worth bearing in mind, *causa latet, vis est notissima.* We may with advantage recollect it when we seek and find the Force that produces a direct Effect, but stand in utter perplexity before the manifold and transcendent meanings of that great word "cause."

The similarity between organic forms and those that physical agencies are competent to produce still leads some men, such as Stéphane Leduc, to doubt or to deny that there is any gulf between, and to hold that spontaneous generation or the artificial creation of the living is but a footstep away. Others, like Delage and many more, see in the contents of the cell only a complicated chemistry, and in variation only a change in the nature and arrangement of the chemical constituents; they either cling to a belief in "heredity," or (like Delage himself) replace it more or less completely by the effects of functional use and by chemical stimulation from without and from within. Yet others, like Felix Auerbach, still holding to a physical or quasi-physical theory of life, believe that in the living body the dissipation of energy is controlled by a guiding principle, as though by Clerk Maxwell's demons; that for the living the Law of Entropy is thereby reversed; and that Life itself is that which has been evolved to counteract and battle with the dissipation of energy. Berthold, who first demonstrated the obedience to physical laws in the fundamental phenomena of the dividing cell or segmenting egg, recognises, almost in the words of John Hunter, a quality in the living protoplasm, *sui generis*, whereby its maintenance, increase, and reproduction are achieved. Driesch, who began as a "mechanist," now, as we have

[1] Cf. Giard, "Discours inaugurale," *Bull. Scientif.* (3), 1, 1888.

[2] "Ich behaupte aber dass in jeder besonderen Naturlehre nur so viel *eigentliche* Wissenschaft angetroffen werden könne, als darin Mathematik anzutreffen ist."—Kant, in Preface to "Metaphys. Anfangsgründe der Naturwissenschaft" (Werke, ed. Hartenstein, vol. iv. p. 360).

seen, harks back straight to Aristotle, to a twin or triple doctrine of the soul. And Bergson, rising into heights of metaphysics where the biologist, *qua* biologist, cannot climb, tells us (like Duran) that life transcends teleology, that the conceptions of mechanism and finality fail to satisfy, and that only "in the absolute do we live and move and have our being."

We end but a little way from where we began.

With all the growth of knowledge, with all the help of all the sciences impinging on our own, it is yet manifest, I think, that the biologists of to-day are in no self-satisfied and exultant mood. The reasons and the reasoning that contented a past generation call for reinquiry, and out of the old solutions new questions emerge; and the ultimate problems are as inscrutable as of old. That which, above all things, we would explain, baffles explanation; and that the living organism is a living organism tends to reassert itself as the biologist's fundamental conception and fact. Nor will even this concept serve us and suffice us when we approach the problems of consciousness and intelligence and the mystery of the reasoning soul; for these things are not for the biologist at all, but constitute the Psychologist's scientific domain.

In Wonderment, says Aristotle, does philosophy begin,[1] and more than once he rings the changes on the theme. Now, as in the beginning, wonderment and admiration are the portion of the biologist, as of all those who contemplate the heavens and the earth, the sea, and all that in them is.

And if Wonderment springs, as again Aristotle tells us, from ignorance of the causes of things, it does not cease when we have traced and discovered the proximate causes, the physical causes, the Efficient Causes of our phenomena. For beyond and remote from physical causation lies the End, the Final Cause of the philosopher, the reason Why, in the which are hidden the problems of organic harmony and autonomy and the mysteries of apparent purpose, adaptation, fitness, and design. Here, in the region of teleology, the plain rationalism that guided us through the physical facts and causes begins to disappoint us, and Intuition, which is of close kin to Faith, begins to make herself heard.

And so it is that, as in Wonderment does all philosophy begin, so in Amazement does Plato tell us that all our philosophy comes to an end.[2] Ever and anon, in presence of the *magnalia naturae*, we feel inclined to say with the poet:

οὐ γάρ τι νῦν τε κἀχθὲς, αλλ' ἀεί ποτε
ζῇ ταῦτα, κοὐδεὶς οἶδεν ἐξ ὅτου 'φάνη.

"These things are not of to-day nor yesterday, but evermore, and no man knoweth whence they came."

I will not quote the noblest words of all that come into my mind, but only the lesser language of another of the greatest of the Greeks: "The ways of His thoughts are as paths in a wood thick with leaves, and one seeth through them but a little way."

SECTION E.

GEOGRAPHY.

OPENING ADDRESS BY COLONEL C. F. CLOSE, C.M.G., R.E., PRESIDENT OF THE SECTION.

I PROPOSE to devote the first part of this address to an examination of the purpose and position of Geography, with special reference to its relations with other subjects. It will not be possible entirely to avoid controversial matters; but, if some of the questions touched on are controversial, this only means that these questions have a certain importance. I shall try to describe the facts of the case impartially.

In the second part I shall try to indicate briefly what the Government, as represented by the great Departments of State, is doing for Geography.

PART I. *The Position of Geography with reference to other Subjects.*

It is no secret that the geographical world is not unanimous about the meaning and object of Geography. The definitions suggested by such writers as Mr. Chisholm, Prof. Davis, Prof. Herbertson, Mr. Mackinder, or Dr. Mill, are not in agreement. From time to time an attempt is made to formulate some statement which shall not commit the subscribers to anything very definite. But differences of opinion on the subject persist.

There are, of course, a great many ways of approaching the question. Let us, for example, examine the proceedings of such representative bodies as the British Association and the Royal Geographical Society, and of such assemblies as the International Geographical Congresses, and let us see if we can find out what is, as a fact, the scope of the subject as dealt with by these bodies. They are institutions which work in the full light of day, and they are too large to be dominated for any length of time by individuals. If we can find any working principle, any common term, amongst these societies, we shall have gone some way towards arriving at a solution of the problem.

A simple method of investigation is to discuss the character of the publications of these societies and of the lectures delivered before them. And I feel that I cannot do better than devote most of this brief analysis to the Royal Geographical Society and its admirably edited Journal. Here we are on safe ground. If an inhabitant of another planet wished to know what we understand by astronomy we could confidently refer him to the Monthly Notices of the Royal Astronomical Society. If he were curious about the condition of geology, we should give him the volumes of the Geological Society. And, if he were so rash as to ask what are the objects of the modern mathematician, we should hand him the papers published by the London Mathematical Society. The "Geographical Journal" occupies no lower a position with reference to Geography than do the other journals mentioned with reference to the sciences with which they deal.

In analysing the contributions to the Royal Geographical Society, it is important to start with an honest classification. In the endeavour to be impartial I have chosen the classification which was adopted for the last International Geographical Congress, *i.e.* that held at Geneva in 1908. This Congress was divided into fourteen sections. It will serve to clear the ground if we deal first with sections 12, 13, and 14; these are the Teaching of Geography, Historical Geography (which was mainly concerned with the history of travel and exploration), and Rules and Nomenclature. For the purpose of discovering what Geography is, these three sections will not be of any assistance. Every subject has its educational side, its history, and its rules and nomenclature. The subject proper was, therefore, divided into eleven sections. The eleven sections are the following:—

(1) Mathematical and Cartographical Geography.
(2) General Physical Geography.
(3) Vulcanology and Seismology.
(4) Glaciers.
(5) Hydrography (Potamography and Limnology).
(6) Oceanography.
(7) Meteorology and Climatology; Terrestrial Magnetism.
(8) Biological Geography.
(9) Anthropology and Ethnography.
(10) Economic and Social Geography.
(11) Explorations.

Before applying this classification to the work of the Geographical Society, I wish to call attention to the extremely frank way in which vulcanology, seismology, meteorology, climatology, terrestrial magnetism, anthropology, and ethnography are included in Geography. The list, in fact, covers ground occupied by several sections of the British Association.

I have investigated the work of the Geographical Society for the five complete years 1906 to 1910. The original contributions to the "Geographical Journal" have been examined for that period, omitting from consideration contributions on the subjects of teaching, the history of exploration, and rules and nomenclature.

There are altogether 296 original papers which come under one or another of the eleven headings given above. Of these papers, 171, or 57 per cent., deal with Explorations and Travels. There is a great drop to the next largest section, General Physical Geography, which

[1] "Met." I., 2,982b, 12, &c.
[2] Cf. Coleridge, "Biogr. Lit."

accounts for thirty papers, or about 10 per cent. Adhering to the order of the Geneva Congress, the complete list is as follows:—

Original Contributions to the Proceedings of the Royal Geographical Society during the five years 1906 to 1910.

Subject	Percentage
(1) Mathematical and Cartographical Geography	3
(2) General Physical Geography	10
(3) Vulcanology and Seismology	5
(4) Glaciers	3
(5) Hydrography (Potamography and Limnology)	5
(6) Oceanography	3
(7) Meteorology and Climatology; Terrestrial Magnetism	3
(8) Biological Geography	1
(9) Anthropology and Ethnography...	3
(10) Economic and Social Geography	7
(11) Explorations	57

The main conclusion is obvious enough. For the principal Geographical Society in the world, Geography is still mainly an affair of explorations and surveys; if to this item we add cartography, we account for 60 per cent. of the activities of the Society.

There is another important deduction which is natural and unforced: the papers on vulcanology and seismology and on glaciers could have been read with perfect appropriateness before the Geological Society; those on meteorology and climatology before the Meteorological Society; and those on anthropology and ethnography before the Anthropological Society. To make quite sure of this point I will cite a few titles of the papers read: "The Great Tarawera Volcanic Rift," by J. M. Bell; "Recent Earthquakes," by R. D. Oldham; "Glacial History of Western Europe," by Prof. T. G. Bonney; "Climatic Features of the Pleistocene Ice Age," by Prof. A. Penck; "Rainfall of British East Africa," by G. B. Williams; "Geographical Distribution of Rainfall in the British Isles," by Dr. H. R. Mill; "Geographical Conditions affecting Population in the East Mediterranean Lands," by D. G. Hogarth; "Tribes of North-Western Se-Chuan," by W. N. Fergusson.

This little list of typical subjects indicates clearly that there is a large group of contributions which would have found an appropriate home in the journals of the Geological, Meteorological, and Anthropological Societies; there is a possible corollary that, since men who make a life-study of these subjects are best capable of dealing with them, the authors of the above type of paper who submit their work to the Geographical Society in so doing appeal rather to the public at large than to men of their own special sciences.

We may therefore sum up the results of this brief investigation into the work of the Royal Geographical Society by saying that 60 per cent. of it is concerned with exploration and mapping, and that some of the remainder could be dealt with appropriately by the learned societies concerned, but that the Geographical Society serves as a popularising medium. It also serves a useful purpose as a common meeting-ground for vulcanologists, seismologists, oceanographers, meteorologists, climatologists, anthropologists, and ethnographers.

Another line of investigation may be profitably pursued. Who are, by common consent, the leading geographers of the world? No doubt the explorers come first in popular estimation, such men (omitting British names) as Peary, Charcot, Sven Hedin. Then after this type would come the men of learning who stand out in any International Congress. These men stand out because they have, by their own exertions, increased the sum of human knowledge. Omitting, for the moment, the consideration of exploration and mapping, we find that in an international congress a large number of the most celebrated geographers are eminent as geologists. In such a gathering we can also pick out those who have advanced the sciences of meteorology or anthropology. Now, suppose the position reversed. Let the functions of geology be supposed to be somewhat in dispute and those of geography perfectly definite, and further let us suppose that at an international meeting of geologists a large proportion of the men of real distinction were geographers. We may in this way get an idea of what geography looks like from the outside.

I think that at this point we may explain, in a preliminary way, the work of the Geographical societies, after the fashion of the "Child's Guide to Knowledge":—

Question. What is Geography?

Answer. There is no generally accepted definition of Geography.

Question. Can we not form some idea of the scope of the subject by considering the work of the Royal Geographical Society?

Answer. Yes; 60 per cent. of this work deals with explorations, surveys, and mapping, and of the rest a considerable portion consists of matter which could be discussed appropriately before the Geological, Meteorological, and Anthropological Societies.

Question. What, then, leaving maps out of consideration, are the useful functions of a Geographical society?

Answer. A Geographical society serves to popularise the work of men who labour in certain fields of science, and such a society forms a very convenient meeting-ground for them.

Question. What is a geographer?

Answer. The term geographer is sometimes applied to explorers; sometimes to men who compile books derived mainly from the labours of surveyors, geodesists, geologists, climatologists, ethnographers, and others; sometimes to those who compile distributional maps.

Question. Can a geographer who has not made a special study of one or more of such subjects as geodesy, surveying, cartography, geology, climatology, or ethnography, hope to advance human knowledge?

Answer. He can do much to popularise these subjects, but he cannot hope to do original work.

Another way of attempting to ascertain the meaning and object of Geography is to study the character of the instruction given in the universities, and we may suppose that this can be fairly judged by the contents of standard text-books. Let us take, for example, the "Traité de Géographie Physique" of M. E. de Martonne, formerly Professor of Geography at the University of Lyons, now Professor at the Sorbonne. The work in question was published in 1909, and is divided into four main sections—Climate, Hydrography, Terrestrial Relief, and Biogeography.

The first sentence of the book is "What is Geography?" Twenty-four pages are devoted to discussing this question, which the writer, with all his skill and learning, finds it difficult to answer definitely and convincingly. One receives the impression of the dexterous handling of a difficult question, and of a generally defensive attitude. In this book geography is said to depend on three principles. The principle of *extension*, the principle of *coordination*, and the principle of *causality*. As an illustration of the meaning of the principle of extension, we are told that "the botanist who studies the organs of a plant, its conditions of life, its position in classification, is not doing geographical work; but if he seeks to determine its area of extension, *il fait de la géographie botanique.*" I believe that we have here reached a critical point. The claim is, that when, in the prosecution of a botanical study, a map is used to show the distribution of a plant, the use of such a map converts the study into a branch of geography. Well, it is a question of definition and convention, which cannot, I imagine, be settled except by the general agreement of all the sciences. We have to make up our minds whether a man who constructs a distributional map is doing "geography." One thing, I suppose, is not doubtful. When the map is made it will be better interpreted by a botanist than by a person ignorant of botany. In the same way the discussion of an ordinary geological map is best undertaken by a geologist, and so on. It would appear that geography, in the sense mentioned, is not so much a subject as a method of research.

It will be convenient here to say a few words about the relations between societies and schools of Geography, and those two important subjects geodesy and geology. Curiously enough, there is not, and has never been, in the United Kingdom a society or body specially charged with the study of geodesy. Geodesy, in fact, has no

regular home in these islands. But the Royal Geographical Society has done a good deal in the past few years to stimulate an interest in the subject, thereby fulfilling what I believe to be one of the Society's most useful functions, that of popularisation.

If, however, an authoritative opinion were required on any geodetic question, where could it be obtained? Well, I suppose there is no doubt that the headquarters of this branch of learning is the International Geodetic Association; but the scientific work itself is being largely carried out at the Geodetic Institute at Potsdam, by the Survey of India, by the Geodetic Section of the Service Géographique, by the U.S. Coast and Geodetic Survey, and by similar bodies. Geodesy, especially in its later developments, is a definitely scientific subject which demands much study and application. It is but slightly touched upon by the schools of Geography. Perhaps I may here point out that geodesy is by no means mainly concerned with the shape of the spheroid. The chief problems are now those of isostasy and local attraction generally, the real shape of the sea-surface, the continuity of the crust of the earth, and changes of density therein.

The position in which Geography finds itself with regard to Geology can be clearly seen if reference is made to the new edition of the "Encyclopædia Britannica." In the eleventh volume of this work are two important articles, "Geography," by Dr. H. R. Mill, and "Geology," by Sir Archibald Geikie. In the article on "Geography" we find a description of geomorphology as that part of Geography which deals with terrestrial relief, and a remark is made that "opinion still differs as to the extent to which the geographer's work should overlap that of the geologist." In this article, however, most of the authorities quoted are geologists, and the author remarks that "the geographers who have hitherto given most attention to the forms of the land have been trained as geologists."

Turning to the article on "Geology," we find an important section on "Physiographical Geology," which is described as dealing with the investigation of "the origin and history of the present topographical features of the land." Now this is the exact field claimed for geomorphology. It has been observed by others, notably by Prof. de Martonne, that the interpretation of topographic forms has been most successfully undertaken by geologists, and he gives as an instance of this the good work done by the United States Geological Survey.

I do not know whether any geographer untrained as a geologist has contributed anything of value to geomorphology.

Another test which may be applied is the following: Let us imagine Geography to be non-existent, and note what the effect would be. Suppose there were no such things as Government Geographical Services, or Schools of Geography at the Universities, or Geographical Societies. The first and most obvious result would be that most, if not all, of our apparatus of exploration and mapping would have disappeared. But as we are all in agreement as to the necessity of this branch of human effort, let us restore this to existence and examine the effect of the disappearance of the rest.

So far as concerns geodesy, we should still possess the International Geodetic Association, the Geodetic Institute at Potsdam, and the United States Geodetic Survey, and similar bodies. But we should have lost the means of popularising geodesy in the proceedings of Geographical Societies; and, as there would be now no geographical text-books, elementary geodesy would not find itself between the same covers as climatology and geomorphology.

As regards geomorphology, or physiographical geology, not very much difference would be noted. The geologists would still pursue this important subject; but here again their writings would perhaps appeal to a more expert and less popular audience, although it is not to be forgotten that many admirable introductions to the subject have been written by geologists.

Much the same might be said about meteorology and climatology. There would be text-books devoted to these studies, but there might be a diminution of popular interest.

Such names as phyto-geography would disappear, but the study of botany (if we permit it the use of distributional maps) would not be affected. The loss to knowledge would be mainly that of getting to a certain extent out of touch with the public. The constitutions of the various learned bodies would remain the same, and so would their functions. The constitution of the Royal Society, which has never recognised geography as a subject, would be totally unaffected.

If we thus study the relations between Geography and other subjects we are almost bound to arrive at the conclusion that Geography is not a unit of science in the sense in which geology, astronomy, or chemistry are units. If we inquire into the current teaching of Geography, and examine modern text-books, we find that most of the matter is derived directly from the workers in other fields of study. And if we inquire into the products of Geographical societies, it becomes evident that one of the most important functions fulfilled by these useful bodies is to popularise the work of geodesists, geologists, climatologists, and others, and to provide a common meeting-ground for them. If Geography had been able to include geology and the other sciences which deal with earth-knowledge, it would then, indeed, have been a master science. But things have worked out differently.

I shall very probably be told that, in laying some stress on the above-mentioned aspects of the subject, I have forgotten that the main purpose of Geography is the study of the earth as the home of man, or the study of man as affected by his environment, and that, however necessary it may be to begin with a foundation of geodesy, geology, and climatology, we must have as our main structure the investigation of the effect of place conditions on the races of man, on human history and human industry, on economics and politics.

It is obviously and abundantly true that no student of history, economics, or politics can disregard the effect of geographical environment. But it is not, as a fact, disregarded by writers on these subjects. The question is, to a large extent, whether we should annex these portions of their studies, group them, and label them "Geography." Our right to do this will depend on the value of our own original investigations. We have the right to use the results obtained by others, provided that we add something valuable of our own.

Before this human aspect of geography—or, for that matter, any other aspect of the subject—is recognised by the world of science as an independent, indispensable, and definite branch of knowledge, it must prove its independence and value by original, definite, and, if possible, quantitative research.

PART II. *Geography and the Government Departments.*

Whatever definition of Geography is accepted, we are all in agreement that the map is the essential foundation of the subject. I propose now to indicate very briefly how the British Government, as represented by the great Departments of State, is, in this respect, assisting the cause of Geography. The Departments which are interested in maps and surveys are the following:—The Admiralty, the War Office, the Colonial Office, the India Office, the Board of Agriculture, and the Foreign Office.

The immense services rendered, not only to this country, but to the whole world, by the Hydrographic Department of the Admiralty are known to all. But it would be somewhat rash of a soldier to talk about hydrographic surveys, so I will confine my remarks to surveys on land.

First, it should be remarked that the British Government as a whole has for many years shown its interest in Geography, and has recognised the good work done by the Royal Geographical Society by contributing an annual sum of 500*l.* towards the funds of the Society. Next, it should be noted that from time to time British Governments have contributed large sums of money towards Arctic and Antarctic exploration. The most recent examples of this very practical form of encouragement will be remembered by all; I mean the Government expenditure on Scott's first Antarctic Expedition and the handsome sum contributed towards the cost of Shackleton's great journey.

Turning now to the *War Office*, the first matter to which I would call attention is that nearly all the accurate topographical surveys of the Empire have been started by soldiers. This applies to the United Kingdom, Canada, Australia, South Africa, Tropical Africa, and last, but greatest of all, India. The accounts of the struggles of soldiers at the end of the eighteenth century to obtain sanction for what is now known as the Ordnance Survey form very interesting reading. In fact, all over the world it was military requirements which produced the topographical map; and it is still the War Offices of the world which control the execution of almost all geographically important surveys. During the last few years the largest block of work undertaken by the War Office has been the accurate survey of the Orange Free State, which has an area of about 52,000 square miles—nearly the size of England—and an adjacent reconnaissance survey in the Cape of Good Hope covering an area of a hundred thousand square miles. There has been some inevitable delay (due to causes which need not be gone into now) in the publication of the sheets of this survey, but the work is being pushed on. The survey of the Orange Free State is fully comparable with the admirable surveys carried out by the French Service Géographique de l'Armée in Algeria and Tunis. Some work has also been done in the Transvaal. Other surveys carried out in recent years under the direct control of the War Office are those of Mauritius, St. Helena, a portion of Sierra Leone, Malta, and Hong Kong.

The most notable work which is now being carried out in the *Self-Governing Dominions* is the Militia Department Survey of Canada, with which excellent progress has been made.

The total area of the Crown Colonies and Protectorates under the rule of the *Colonial Office* amounts to about two million square miles. British African Protectorates form a large portion of this total, and I will indicate briefly what is being done to survey these tropical Protectorates. From the geographical point of view the brightest regions are East Africa, Uganda, and Southern Nigeria. In East Africa topographical surveys of the highlands and coast belt are being pushed on by military parties as part of the local survey department. The area of exact work done amounts now to some 30,000 square miles. In Uganda a military party has recently completed a large block of country, and in this Protectorate thoroughly trustworthy maps of 32,000 square miles are now available. In Southern Nigeria a completely reorganised survey department is tackling in a thoroughly systematic fashion the difficult task of mapping a forest-clad country. We shall shortly see the results.

For the information of those who have not travelled in Tropical Africa it should be remarked that surveying in such countries is attended by every sort of difficulty and discomfort, and too often by illness and serious discouragement. It is one thing to sit at home in a comfortable office and plan a scheme of survey, and quite another thing to carry it out on the spot. We do not, I am convinced, give enough honour and credit to those who actually get the work done in such trying circumstances. Honest, accurate survey work in the tropics puts a much greater strain on a man than exploratory sketching. To picture what the conditions are, imagine that you are to make a half-inch survey of the South of England: cover the whole country with dense forest; put mangrove swamps up all the estuaries; raise the temperature to that of a hot-house; introduce all manner of insects; fill the country with malaria, yellow fever, blackwater fever, and sleeping sickness; let some of your staff be sick; then have a fight with the local treasury as to some necessary payment, and be as cheerful as you can. That is one side of the medal. On the other side there is the abiding interest which the surveyor feels in the country, the natives, and the work; the sense of duty done; and the satisfaction of opening up and mapping for the first time a portion of this world's surface.

There is no time to mention other surveys in Africa, and I will pass on to a very interesting part of the world, the Federated Malay States. In this prosperous country much excellent geographical work is being done by the combined survey department which was established under a Surveyor-General in the year 1907. The department is in good hands, and the commencement of a regular topographical series is being undertaken.

I wish it were possible to prophesy smooth things about Ceylon. From our special point of view the situation leaves much to be desired. There is not yet published a single topographical map, and the topographical surveys are progressing at a rate which, under favourable conditions, may result in the maps being completed in the year 1970.

In closing this inadequate review of the principal surveys which are being undertaken in the Crown Colonies and Protectorates, I should mention that the coordinating factor is the Colonial Survey Committee, which every year publishes a report which is presented to Parliament.

The *India Office* is, of course, concerned with that great department the Survey of India. The Indian Empire has an area of about 1,800,000 square miles, and as, under the arrangements approved in 1908, the standard scale of survey is to be one inch to one mile, the area of paper to be covered will be 1,800,000 square inches. Actually this is divided into about 6700 sheets. The Survey of India has always been famous for its geodetic work and for its frontier surveys and methods. Its weak point used to be its map reproduction. This has been greatly improved. But personally I feel that if, for most military and popular purposes, a half-inch map is found suitable for England, as is undoubtedly the case, there is no reason why a half-inch map should not also be suitable for India. It is mainly a question of putting more information on the published map, and of engraving it and using finer means of reproduction. If this smaller scale were adopted all the information now presented could be shown, and the number of the sheets would be reduced from 6700 to 1675, a saving of 5000 sheets. It is difficult to avoid the feeling that the Survey of India is over-weighted with the present scheme. The scheme has, however, many merits. It will be impossible to carry it out unless the department is kept at full strength.

The *Board of Agriculture* is the Department which is charged with the administration of the Ordnance Survey. The Ordnance Survey spends some 200,000*l.* a year, and for that sum it furnishes the inhabitants of the United Kingdom with what are, without doubt, the finest and most complete series of large-scale maps which any country possesses. There is nothing in any important country (such as France, Germany, Italy, Russia, or the United States) to compare with our complete and uniform series of sheets on the scale of $\frac{1}{2500}$. These sheets are sold at a nominal price, and are, in effect, a free gift to landowners, agents, and all who deal with real property. They are also, of course, invaluable to county and borough engineers and surveyors. They really are a national asset which is not half enough appreciated. The whole conception of these large-scale plans has stood the test of time, and is greatly to the honour of a former generation of officers.

Much might be said about the small-scale maps of the Ordnance Survey, which are now published in a very convenient form. As mentioned below, the latest small-scale Ordnance map is the new international map on the million scale. Some sheets of this map will shortly be published.

The *Foreign Office* is concerned with the surveys of the Anglo-Egyptian Sudan, which are at present mainly of an explanatory character. The taking over of the Province of Lado has recently thrown fresh work on the Sudan Survey Department. The Foreign Office, which administers Zanzibar, has recently given orders for the survey of the Island of Pemba, a dependency of Zanzibar, and this is being carried out by a small military party.

But the greatest service to Geography rendered by the Foreign Office in recent years was the encouragement given to the project of the International Map by the assembly of an international committee in November, 1909. Sir Charles (now Lord) Hardinge presided at the opening session. There were delegates from Austria-Hungary, France, Germany, Great Britain, Canada and Australia, Italy, Russia, Spain, and the United States, and, as is known, the resolutions which were devised by the Committee were agreed to unanimously. After the conclusion of the work of the Committee the Government communi-

cated the resolutions to all countries which had not been represented, and nearly all the replies which have been received are favourable. Maps in exact accordance with the resolutions are, it is understood, being produced by France, Hungary, Italy, Spain, the United States, and other countries, and, so far as we are concerned, by the General Staff and the Ordnance Survey. These maps will be shown at the International Geographical Congress which meets at Rome in October next.

I have now come to the end of this rapid sketch of the geographical work of the official world. It is work which, though often of an apparently humdrum character, outweighs in importance the sum total of all which can be undertaken by private agency or by societies. But it is the very legitimate business of societies to criticise and encourage.

It is, in fact, not only our manifest duty to encourage the systematic mapping of the world on which we live, but we should do all we can to ensure the perfection, and suitability for their special purposes, of the maps themselves. In the surveying of the earth's surface and its representation by means of maps we are treating of matters which are essentially and peculiarly our own.

It would appear that another great function of Geography, as represented by Geographical societies and congresses, is to serve as a popularising medium for such sciences as geodesy, geology, climatology, and anthropology, and also to serve as the means of bringing together the workers in these sciences. We may be told that, so far as this Association is concerned, the exact study of geodesy and meteorology is dealt with by Section A, geology by Section C, and anthropology by Section H, but there is, I believe, no other section which forms a more convenient general meeting-ground for all the workers in the various divisions of earth-knowledge. We ourselves have our own special work, work which is shared by no others, the great task of mapping the world. This task is such a necessary one, and it is of such genuine value to so many studies, that by assisting in it we are really furthering the Advancement of Science, which is the object of this great Association.

NOTES.

COLONEL C. F. CLOSE, C.M.G., R.E., has been appointed Director-General of the Ordnance Survey in succession to Colonel S. C. N. Grant, C.M.G., R.E.

DR. F. GRANT OGILVIE, C.B., has been appointed by the President of the Board of Education director of the Science Museum. He will continue to fill his present position of secretary for the Science Museum and the Geological Survey and Museum.

THE memory of Alessandro Volta was honoured on Friday last by a meeting held at his grave at Camnago, at which were present Signor Calissano, Minister of Posts and Telegraphs, M. Buebs, director of the Belgian telegraphs, Signor Pietro Volta (a nephew of the inventor), and many telegraphists from all parts of the world. Several speeches were delivered, and a memorial stone bearing an inscription recording the esteem in which Volta is held was inaugurated. The ceremony was followed by a luncheon, provided by the Mayor, and the placing of wreaths, at Como, on the Volta monument.

THE Brussels correspondent of *The Times* announces the death of Prof. F. Swarts, the holder for many years of the chair of general chemistry at the University of Ghent.

THE death is announced, at the age of sixty-four years, of Mr. T. Hurry Riches, locomotive superintendent of the Taff Vale Railway. Mr. Riches, who was held in high esteem as an engineer, for three successive years filled the presidency of the South Wales Institute of Engineers. He was also an ex-president of the Institute of Mechanical Engineers, a member of the Iron and Steel Institute, of the British Association, of the council of the University College of South Wales and Monmouthshire, of the council of governors of the Imperial College of Science and Technology, of the council of the Institute of Metals, and a governor of the National Museum of Wales. He served as chairman of the mechanical engineering section of the Franco-British Exhibition, and as reporter for Great Britain and the colonies to the International Railway Congress of 1910 upon railway motor-cars.

DR. GILMAN A. DREW, assistant director of the Woods Hole Marine Biological Laboratory, has been appointed resident assistant director of the laboratory, and will in future devote his whole time to the work at Woods Hole.

IT is announced in *Science* that Prof. A. J. Hitchcock, of the United States Department of Agriculture, has left for Panama to join the Smithsonian expedition for the biological survey of the Panama Canal zone. He will also investigate the grasses of the five Central American Republics on behalf of the department with which he is connected.

ACCORDING to advices received at Cordova, Alaska, the Smithsonian Institution's glacial expedition has had an unlucky accident. As Profs. R. F. Starr and Lawrence Madden were crossing the Yukon on their way to Fairbanks, their wagon was upset by the current. The explorers themselves got ashore in safety, but all their field notes, cameras, and exposed films were lost.

The Electrician, quoting from the French official gazette "L'Officiel," states that a committee dealing with the hygienic aspects of illumination has been appointed by the Minister of the Interior in France. The objects of the committee include the general effects of illumination on health, the framing of simple rules as to the best means of applying customary systems of lighting to various industrial operations, the nature and causes of short sight and impairment of vision, and their connection with defective living conditions, the study of methods of measuring illumination, &c.

A BRUSSELS correspondent of *The Times* states that a special commission was recently appointed to study the utilisation of aëroplanes for ensuring rapid communication with districts of the Belgian Congo that are still unprovided with railways and roads, and that it has been decided to await the results of certain tests to be carried out in France. Attempts will be made to traverse a desert about 1200 kilometres (750 miles) across, and to establish landing stations 400 kilometres apart, fitted with wireless telegraphy. The aëroplanes will have to convey three passengers and a relatively heavy load of victuals, water, tools, &c. It is hoped that this line will be established in 1912. A first subsidy of 16,000*l*. has been voted for the establishment of these communications.

ACCORDING to a Reuter telegram, the acting Russian Consul at Kwang-cheng-tse reports the outbreak at Changchun, Manchuria, of an unknown disease. The sufferers are attacked by headache and violent diarrhœa, and lose the power of speech. Death occurs in two or three days. The Chinese and Japanese doctors are, it is said, doubtful of the nature of the disease.

THE Board of Agriculture and Fisheries has decided to appoint a departmental committee to inquire into the circumstances of the recent outbreaks of foot-and-mouth disease, and to consider whether any further measures can be adopted to prevent their recurrence. The committee will be appointed and sit in the autumn under the chairmanship of Sir Ailwyn Fellowes.